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ILLUSTRATIONS OF

THE VISCOUS THEORY

OF

GLACIER MOTION.

PART I.—CONTAINING EXPERIMENTS ON THE FLOW OF PLASTIC BODIES, AND OBSERVATIONS ON THE PHENOMENA OF LAVA STREAMS.

PART II.—AN ATTEMPT TO ESTABLISH BY OBSERVATION THE PLASTICITY OF GLACIER ICE.

PART III.—ILLUSTRATIONS OF THE VISCOUS THEORY OF GLACIER MOTION.

BY

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XII. *Illustrations of the Viscous Theory of Glacier Motion.*

Part I. *Containing Experiments on the Flow of Plastic Bodies, and Observations on the Phenomena of Lava Streams.*

By JAMES D. FORBES, *Esq., F.R.SS. L. and E., Corresponding Member of the Institute of France, and Professor of Natural Philosophy in the University of Edinburgh.*

Received March 15,—Read April 10, 1845.

§ 1. *Plastic Models.*
§ 2. *Analogy of Glaciers to Lava Streams.*
Note on the Velocity of Lava.

§ 1. *Plastic Models.*

IN the concluding chapter of my "Travels in the Alps of Savoy," I have shown how the obscure relations of the parts of a semifluid or viscous mass in motion (such as I have attempted to prove that the glaciers may be compared to) may be illustrated by experiment.

The larger models, these described and figured, showed very clearly the precise effects of friction upon the motion of such a mass. They were formed of plaster of Paris, mixed with glue, and run in irregular channels, and the relative velocities of the top and bottom, the sides and centre of such a pasty mass were displayed by the alternating layers of two coloured pastes, which were successively poured in at the head of the model valleys. The boundaries of the coloured pastes were squeezed by the mutual pressures into greatly elongated curves whose convexity was in the direction of motion; and in a vertical medial section, the retardation of the bottom and the mutual action of the posterior and anterior parts, shaped the bounding surface of two colours into a spoon-like form.

Now these models convey a very palpable commentary upon the effects of friction on a plastic mass, and likewise on the influence of the mutual pressures of its parts; but in further illustration of the same thing I constructed another model, only executed as the printing of my volume approached its close, and which is cursorily described in a long note (page 377)*, whence its real importance may perhaps have been pretty generally overlooked.

The models in question, of which I have since made many, are formed by accumulating in one end of a long narrow box, AB, Plate IV. fig. 1, a deep pool of the viscid

* In this paper reference is of course made to the first edition of my "Travels," the second not having been then published.

material already mentioned, which is retained there by a sluice or partition C which may be withdrawn at pleasure.

The surface of the pool *abcd* is then pretty thickly dusted over with a coloured powder, and the sluice is withdrawn.

The pasty mass subsides slowly under its own weight into the lengthened form *efgh*. The film of colour on the surface is therefore broken up so as to cover three or four times the surface it did at first; and its new distribution marks the lines of greatest separation of the superficial particles of the mass. The appearance of such a model when *run* is shown in fig. 2, and it manifests in the plainest manner the twofold tendency to separation in such a case where the channel is narrow and confined, and there is a certain mass of matter in front. Plate V. shows a more accurate drawing taken from such a model.

The lines of *sliding* separation occur most distinctly marked near the sides, where the friction is greatest, and the central parts are *forced past* the lateral parts, on account of the less embarrassed and consequently swifter motion of the centre; and they incline to the centre although the breadth of the channel be perfectly uniform. But the forces which tear asunder the parts (when such exists) act *perpendicularly to the former* and produce dislocations and fissures, which perfectly correspond to the direction and appearance of the crevasses of a glacier, that is, they are convex upwards or towards the origin of the glacier. It is the former of these lines of separation, or *differential motion*, which constitute and trace out with an exact parallelism the *veined structure* which I have described as forming the normal structure of all true glaciers. Plate V. is a representation of a very beautiful plaster model of more consistence than the other, in which the swelling of the surface and the direction of the open cracks produced by direct thrusts are most beautifully shown; and are even more so in the model than in the engraving. The fissures are transverse and slightly convex to the origin in the higher part of the glacier, then gradually turning round they radiate from a centre in the lower part, exactly as in the glacier of Arolla (Travels in the Alps, Plate VI.), and in all similar cases.

The experiment above detailed was suggested to me by studying the ripple of streams of water, which appear to have the same origin: and in very weak currents moving through very smooth and uniform channels (as the chiseled sides of water conduits) the same may be made manifest by throwing a handful of light powder on the surface, which then becomes divided into threads of particles inclined in the manner I have described at a certain angle from the side towards the centre, depending on the velocity of the stream.

The slightest prominence of any kind in the wall of such a conduit, a bit of wood or tuft of grass is sufficient to produce a well-marked ripple-streak, from the side towards the centre, depending upon the sudden and violent retardation of the lateral streamlets and the freer central ones being momentarily edged away from them. The general course of the motion of the particles is, however, scarcely affected by

such a circumstance, for the differential velocities which cause the ripple and the separation, are always small compared to the absolute velocity of the stream; and thus a floating body on the water (just as the moraine on the glacier) perseveres in its course parallel to the side with scarcely any perceptible disturbance. When however the descent is violent and the friction great, floating bodies are gradually drawn towards the centre, and this happens also in exactly the same circumstances to the moraine of the glacier. Plate IV. figs. 3 and 4, shows the relation of the ripple-marks to the channel of a very flat smooth gutter in one of the side streets of Pisa, sketched after heavy rain.

These ripple-marks in water are well seen near the piers of a bridge, or when a post is inserted in a stream and makes a fan-shaped mark in the water cleft by it: such marks have been much neglected by writers on hydraulics; but in one of the most ancient hydraulic treatises, that of Leonardo da Vinci, lately printed from the MS. in the Italian collection of writers on hydraulics, they are very well described and figured. A case parallel to the last-mentioned, where a fixed obstacle cleaves a descending stream and leaves its trace in the fan-shaped tail, is well seen in several glaciers, as in that at Ferpêcle, and the Glacier de Lys on the south side of Monte Rosa, particularly the last, where the veined structure follows the law just mentioned. And I desire here to record that the views just presented as to the origin of the veined structure of ice, were confirmed, but were not suggested, by the experiments on viscous fluids just mentioned. The necessity of the tearing up of a solid mass, if it moved at all in a bed presenting insurmountable resistances on all sides, in directions such as the veined structure presents, was foreseen by me whilst dwelling amongst the glaciers themselves, at a distance from books or the means of experiment. The following extract from my Third Letter to Professor Jameson, written in 1842 from the remote village of Zermatt, contains the substance of all that I have since developed and illustrated at greater length and in different ways rather to meet the difficulties of others, than to confirm what was plainly fixed in my own mind.

"The glacier struggles between a condition of fluidity and rigidity. It cannot obey the law of semi-fluid progression (maximum velocity at the centre, which is no hypothesis in the case of glaciers, but a fact), without a solution of continuity perpendicular to its sides. If two persons hold a sheet of paper so as to be tense, by the four corners, and one move two adjacent corners, whilst the other two remain at rest or move less fast, the tendency will be to tear the paper into shreds parallel to the motion; in the glacier the fissures thus formed are filled with percolated water, which is then frozen. It accords with this view,—1st, that the glacier moves fastest in the centre, and that the loop of the curve described coincides (by observation) with the line of swiftest motion. 2nd. That the bands are least distinct near the centre, for there the difference of velocity of two adjacent stripes parallel to the length of the glacier is nearly nothing; but near the sides, where the retardation is greatest, it is a maximum. 3rd. It accords with direct observation that the *differ-*

ence of velocity of the centre and sides is greater near the lower extremity of the glacier, and that the velocity is more nearly uniform in the higher part; this corresponds to the less elongated forms of the loops in the upper part of the glacier. 4th. In the highest part of such glaciers, as the curves become less bent the structure also vanishes. 5th. In the wide saucer-shaped glaciers which descend from mountain slopes, the velocity being as in shallow rivers nearly uniform across their breadth, no vertical structure is developed. On the other hand, the friction of the base determines an apparent stratification, parallel to the slope down which they fall. 6th. It also follows immediately (assuming it as a fact very probable, but still to be proved, that the deepest part of the glacier moves slower than the surface) that the *frontal dip* of the structural planes of all glaciers diminishes towards their inferior extremity, where it approaches zero, or even inclines outwards, since then the whole pressure of the semi-fluid mass is unsustained by any barrier, and the velocity varies (probably in a rapid progression) with the distance from the soil; whilst nearer the origin of the glacier the frontal dip is great, because the mass of the glacier forms a virtual barrier in advance, and the structure is comparatively indistinct, for the same reason that the transverse structure is indistinct, viz. that the neighbouring horizontal prisms of ice move with nearly a common velocity. 7th. Where two glaciers unite, it is a fact that the structure immediately becomes more developed. This arises from the increased velocity, as well as friction of each due to lateral compression. 8th. The veined structure invariably tends to disappear when a glacier becomes so crevassed as to lose horizontal cohesion, as when it is divided into pyramidal masses. Now this immediately follows from our theory; for as soon as lateral cohesion is destroyed, any determinate inequality of motion ceases; each mass moves singly, and the structure disappears very gradually*."

In explaining the theory of the veined structure at a meeting of the Royal Society of Edinburgh on the 20th of March 1843, I stated that I had arrived at the conclusion that crevasses resulting from tension in certain parts of a glacier, must be formed at right angles to the surfaces of discontinuity or structural veins where they intersect the surface: a law conformable to the empirical one discovered by me on the glacier of the Rhone in 1841†, since generalized in other cases, and which even the adversaries of my theoretical views have admitted to be a correct statement of the facts‡.

My attention was at that time (March 1843) turned by my learned and acute friend Mr. W. A. Cadell, to the veined structure of the slag of iron furnaces as due to the difference of velocity of the parts producing surfaces of separation and peculiar molecular condition. The transition was easy to the case of volcanic rocks and lava

* Third Letter on Glaciers, Edinburgh Philosophical Journal, October 1842.

† Edinburgh Philosophical Journal, January 1842.

‡ Bibliothèque Universelle, tome xliv. p. 153. "C'est en effet un fait assez général que les bandes bleues coupent à angle droit les crevasses," &c.

streams, and this case was pressed on my attention by an unexpected journey which I soon after undertook to Italy and Sicily.

§ 2. *Analogy of Glaciers to Lava Streams.*

There is something pleasing to the imagination in the unexpected analogies presented by a torrent of fiery lava and the icy stream of a glacier. But when we look upon the comparison historically and critically, and find how generally this analogy has been perceived and adverted to by persons of very different views and talents of observation, we are strongly tempted to suspect that some latent cause confers the marked resemblance.

This cause I of course consider to be the laws and condition of their motion, the struggle of a semi-fluid mass of enormous weight creeping down a mountain side, in which fluidity and solidity are so curiously combined, that we should be at a loss in either case how to name it; a straining, crackling, splintering solid, heaved on by the internal energy of the latent fluidity which pervades it, and which at last succeeds in giving to the general character of the motion and the moving mass, those of fluid bodies subject to the law of gravity; whilst the parts, themselves almost rigid, have that rigidity most fantastically subjected to the action of the dominant principle.

In illustration of what has now been said, I shall quote passages from some authors which, without particular research, have come under my notice expressive of the analogy just mentioned.

Mrs. Starke, the author of a well-known guide book of Italy, published many years ago, speaks of having seen near the crater of Vesuvius in 1818, "five distinct streams of fire issuing from two mouths, and rolling wave after wave slowly down the mountain with the same noise (?) and in the same manner *as the melting glaciers roll into the valley of Chamouni*; indeed this awful and extraordinary scene would have brought to mind the Montanvert, had it not been for the crimson glare and excessive heat of the surrounding scoriæ*."

Mr. Auldjo, an intrepid alpine traveller, writing about Vesuvius, in 1832, says, "The field of lava in the interior of the crater, inclosed within a lofty and irregular bank, might be likened to a lake whose agitated waves had been suddenly petrified; and in many respects resembles the *Mers de Glace*, or level glaciers of Switzerland, although in its origin and materials so very different†." And the view in the same work of "streams of lava on the south-east of the cone" presents a perfect analogy to a glacier, bearing on its surface three medial and two lateral moraines.

Captain Basil Hall, writing of Vesuvius at a later period, uses these remarkable expressions whilst describing an eruption of lava:—"The colour of this stream was a brilliant pink, much brighter at the sides than in the middle, where either from the cooling of the surface, or the accumulation of cinders and broken pieces of stone, a

* Starke's Traveller's Guide, Ninth Edition, p. 293.

† Auldjo's Sketches of Vesuvius, p. 10, published 1833.

sort of dark ridge or backbone was visible from end to end, not unlike the moraine on the top of a glacier. This reminds me of a curious analogy which often struck me, between two objects so dissimilar as a glacier and a lava stream. They are both, more or less, frozen rivers; they both obey the law of gravitation with great reluctance, being essentially so sluggish, that although they both move along the bottoms of valleys with a force well nigh irresistible, their motion is sometimes scarcely perceptible*." This remarkable passage, worded with the usual scrupulous care of the author, combined with his account of the mechanism of a glacier in the description of the glacier of Miage in the same work, show that he had arrived at more correct notions on the subject than any of his contemporaries; notions which chiefly required careful observation to give them the force of demonstration. The allusion to *moraines* as characteristic of lava streams as well as glaciers, in the preceding extract, is perfectly borne out by the view of the lava of 1831 given by Mr. Auldjo and already cited; the same appearance is mentioned by M. Elie de Beaumont in his account of Etna in the following terms: "Une des circonstances que les coulées de lava présentent le plus invariablement * * * * consiste en ce que chaque coulée est flanquée de part et d'autre par une digue de scories accumulées qui rapellent par sa forme la moraine d'un glacier, * * * souvent aussi les coulées présentent de pareilles digues vers leur milieu, lorsqu'elles sont partagées en plusieurs courants distincts coulant l'un à coté de l'autre†."

In another place the same author compares the movement of the upper crust of the lava to that of glaciers according to the then prevalent theory:—"L'écorce supérieure d'une coulée separée de l'écorce inférieure et du sol sousjacent par une certaine épaisseur de lave liquide, ou du moins visqueuse, se trouve dans un état comparable à celui d'un glacier, qui, ne pouvant adhérer au sol sousjacent à cause de la fusion continuelle de sa couche inférieure, se trouve contraint de glisser‡."

Finally, M. Rendu, Bishop of Annecy, in his excellent Essay on Glaciers, refers in one passage (and I believe in one only) to the possible analogy with a lava stream, "[le glacier] s'affaisse-t-il sur lui-même pour couler le long des pentes comme le ferait une lave à la fois ductile et liquide§?"

The following considerations seem to show more than a general external analogy between lava streams and glaciers.

Their velocities are sometimes equally slow. Although common lava is nearly as liquid as melted iron, when it issues from the orifice of the crater, its fluidity rapidly diminishes, and as it becomes more and more burdened by the consolidated slag through which it has to force its way, its velocity of motion diminishes in an almost

* Patchwork, by Captain Hall, vol. iii. p. 118, published 1841.

† Recherches sur le Mont Etna, p. 184. Published in the Mémoires pour servir à une Déscription Géologique de la France, tome iv. 1838.

‡ Ibid. p. 177.

§ Théorie des Glaciers, Mém. de l'Académie de Savoie, tome x. p. 93, published 1841.

inconceivable degree, and at length, when it ceases to present the slightest external trace of fluidity, its movement can only be ascertained by careful and repeated observations, just as in the case of a glacier. In November 1843, I watched lava issuing rapidly from a small mouth in the crater of Vesuvius at the rate of about one foot in a second. The eruption of Etna in 1832 advanced at the rate of five miles in two days, which is at the rate of one foot in about six seconds*. We may contrast with this the eruption of Etna in 1614, which yielded a lava which advanced but two miles in *ten* years according to Dolomieu†, during the whole of which time its motion was sensible. This gives a mean rate of rather more than three feet per day; but at the conclusion it was no doubt much slower.

Mr. Scrope‡ saw the lava of 1819 in the Val del Bove moving down a considerable slope at the rate of a yard a day, nine months after its eruption. It had, he adds, the appearance of a huge heap of rough cinders; its progression was marked by a crackling noise due to friction and straining, and "on the whole was fitted to produce *any other idea than that of fluidity*. In fact," he continues, "we must represent to ourselves the mode in which the crystalline particles of lava move amongst one another, rather as a sliding or slipping of their plane surfaces over each other, facilitated by the intervention of the elastic (?) fluid, than as the rotatory movement which actuates the molecules of most other liquids." It is generally conformable to this view that we find in Hamilton's *Campi Phlegræi* (fol. 1. 38. *Note*) the curious remark that some lava is so incoherent, or whilst fluid has so little *viscosity*, that in issuing from the volcano (Vesuvius) it has appeared "*farinaceous*, the particles separating as they forced their way out, just like meal coming from under the grindstones."

From all this it is quite clear that the seeming rapidity of the parts of a glacier, or the slowness of its motion, cannot be taken as the slightest evidence of its moving otherwise than as a fluid, contending with the *rigor* of the parts which include and resist the moving force, which is truly hydrostatic though limited in its exercise.

It is manifestly futile and unphilosophical to seek one *cause* of motion in a lava which, like that of Vesuvius in 1805, must have described as many hundred feet in a *minute* as that of 1614 from Etna probably did in a *year*§; for the *mean* daily motion of the latter during *ten years* was three feet; but toward the end of that time it must evidently have had for a long period an average motion of one-half or one-quarter of this, and therefore below the observed mean movements of certain glaciers. Fluidity, in the first instance as in the second, was the propelling vehicle or manner in

* E. de Beaumont, Recherches sur le Mont Etna.

† Quoted by E. de Beaumont, p. 85. The original is in the Journal de Physique, vol. i. of the New Series, where it is mentioned that the same slowness of motion has been observed in lavas of Vesuvius. Ferrara (Descrizione del Etna. Palermo, 1818) denies this statement, but not I think on sufficient grounds.

‡ On Volcanoes, p. 102.

§ See Note on the Velocity of Lava Streams at the end of this paper.

which gravity acted, and this is a sufficient answer to any attempt to maintain that the plasticity of a glacier is a collateral but not a primary cause of motion,—a distinction surely without a difference.

As in the case of all imperfect fluids, the central and superficial particles move faster than the lateral and inferior ones; and when the fluidity is *exceedingly* imperfect, as in those long-flowing lavas, there must be a rupture of continuity between the parts to permit them to slide and jostle past one another. This is evidently the cause of the noise referred to by Mr. Scrope and other writers. This tearing up of the stream into longitudinal stripes, occasioned by the varying velocity of the parts, is thus described by M. Dufrenoy in his account of Vesuvius: "La plupart des coulées présentent des bandes longitudinales assez parallèles entre elles: ces larges stries saillantes sur la surface sont les traces du mouvement de la lave qui ne s'avance pas d'une seule pièce, mais par bandes parallèles*."

And M. Elie de Beaumont describes a lava stream at Etna in these terms: "La surface offrait de profondes cannelures parallèles entre elles, dirigées dans le sens du mouvement qui l'avoit déversée à l'extérieur et qui étaient croisées par *de nombreuses gerçures transversales*†." Here then is evidently the twofold system of rents and perpendicular fissures described in the commencement of this paper as being found in the models, and as being conformable to the phenomena of glaciers.

During the winter 1843–44 which I spent in Italy, I had an opportunity of testing these resemblances, and tracing others to glaciers in the lavas of Vesuvius and Etna. I entered on the inquiry with a very jealous care of being drawn into the admission of fanciful or imperfect analogies; and I shall confine myself to the statement of one or two most plain and undeniable confirmations, selected from the results of many fatiguing rambles.

The plastic nature of the viscous lavas of Vesuvius and Etna is such as well might obliterate any internal traces of rents due to differential velocity, which, in the mass, are speedily closed and reunited as in a stream of treacle, or in the plaster models before explained, where the interior is homogeneous and the superficial coating above is permanently dislocated.

In lavas the indescribable ruggedness of the surface very generally prevents any record of the gentler play of forces. The following facts appear to me quite conclusive as to the manner in which a mass partially solidified, yet moving as a fluid, is torn up by the interior forces which act upon it.

1. At Vesuvius, the *Fossa della Vetrana* between the Hermitage and Monte Somma, is a valley lined with the lava of 1751. I here observed that the lava was in some places detached from the wall of the valley, leaving a cavity on the sheltered side of a projecting elbow of rock, just as a glacier does in similar circumstances, showing the considerable consistence which the lava possessed.

* Dufrenoy sur les Environs des Naples, p. 324.

† E. de Beaumont, p. 38.

In the upper part of this Fossa the lava has a distinct linear structure where broken, in shells parallel to the sides, whose thickness varies from one-third of an inch upwards. The position of these surfaces of dislocation is indicated (for illustration) in figure 5 of Plate IV.

2. In the vast lava wastes of Etna, we encounter not only a greater extent of surface, but a greater variety of condition as to cohesion of the lava streams, and the slope down which it has descended, and thus we have a better chance of meeting with specimens of the manner in which the semi-solid crust of a lava stream is torn up and crevassed by the effect of gravity compelling it into the circumstances of fluid motion. From this tendency of all lavas to form slags, and of these slags to be splintered, tossed, and remoulded by the action of the still liquid portion of the stream below or around, not one-thousandth of the surface bears marks of the simple condition of fluidity under which it was originally moulded; and though when viewed from a distance, and in connexion with the form of the ground over which it has passed, we see plainly enough that it has *flowed* like a stream, the absence of any trace of easy undulating forms which characterise fluids or plastic masses, give to the *sciarre* of Etna (the *cheires* of Auvergne) an appearance far more removed from pristine fluidity than the glacier masses of Switzerland.

In traversing many miles of lava wastes between Nicolosi and Zafarana, on the eastern slope of Etna, I met with one singularly favourable specimen of a branch of a stream consolidated exactly as it had moved, and undisturbed afterwards. It is the part of the current of 1763, called *Lava delle Cerve.* The branch stream in question may be ten yards wide, and presents a thin crust, which has floated on the viscid lava below, and which, while yet imperfectly solidified, has been urged to move with the rest of the stream, and has undergone a process of division and rending accordingly. The stream has flowed in the direction from left to right in figure 6. The lateral parts PP, QQ have been *literally torn to pieces longitudinally* (as I wrote on my note-book on the spot) by the multiplied rents which showed the dislocation of the quicker moving central from the lateral parts, and these rents *inclined towards the centre of the stream in the direction in which it moved.* The length of the stream was divided by transverse rents strikingly convex towards the origin of the stream, as shown in the same figure. These cracks were marked by another peculiarity; the cake of floating scoria had not only been cracked across but pushed *upwards,* generally *forwards and upwards,* before it was finally included in the cooling mass of the stream; the result was the arrangement shown in the longitudinal section, fig. 7, which it will be seen resembles the tiling of a house, only that the fractured parts do not always overlap, but the anterior edge is tilted upwards. It will thus be seen that this tendency to separation acts also in the *vertical* plane, and the dotted lines aa', bb', &c. indicate the direction of its action, coinciding with the surfaces of differential motion, which produce what I have called the *frontal dip* of the veined structure of the ice of glaciers.

3. At no great distance from this lava, and near the foot of the hillock called the Serra Pizzuta, between the last-named point and the valley of Tripodo, I observed a *transverse* section of a lava stream exhibiting an arrangement in bands or plates, nearly parallel to the side of the current, but inclining towards the centre.

4. Between Zafarana and the Porta Calanna (Etna), a remarkably pretty illustration occurs in the surface of an old lava stream, worn and polished by the action of a brook. Where the lava has had to turn an abrupt corner of a rock, A, figure 8 (which represents a ground plan), the progress of the lava being violently checked by the resistance of the projecting mass, has been torn up into longitudinal shreds, which from imperfect fluidity have not reunited, but have left open cavities of the form represented in the figure, which exhibit with remarkable fidelity the forms of the fissures with which glaciers are sometimes traversed, when they are subjected to sudden transitions in their states of motion (as in the glacier des Bossons at Chamouni), and which coincide in direction with the veined structure, and pass into it by imperceptible gradations.

5. What I have called the *frontal dip* of the veined structure in glaciers*, I have explained by the accumulation of a sluggish mass of considerable extent upon a floor or bed offering the resistance of intense friction; in consequence of which the mass of ice, urged downwards and forwards by its intense weight, being resisted by the friction of that which immediately precedes it, must yield in the direction of least resistance, or squeeze itself in a slanting direction forwards and *upwards*, and thus sliding over the resisting mass immediately in front, will produce surfaces of discontinuity or differential velocity in that direction. Such a result I inferred from general principles without reference to any particular example, and the explanation of the superficial convexity of the lower part of many glaciers was evidently satisfactorily explained by it.

The convex swelling form of a viscous stream will depend principally upon the relative measure of two quantities, the stiffness or viscosity of the fluid, and the inclination of the surface; although it will also depend on the part of the stream, whether near the origin or the termination, which we consider.

I have found this variation from concave to convex, depending upon circumstances, alike in glaciers and lava streams. Some very highly inclined small glaciers existing at considerable heights, and therefore very hard and consistent, are, nevertheless, deeply concave from end to end, the slope compensating for the stiffness of the matter; such is a beautiful glacier, named, as far as I can learn, La Gria, or Glacier de Bourget, which descends from the Aiguille de Gouté towards the valley of Chamouni. See Plate IV. fig. 9.

Many, perhaps most, lava streams, where they have well-determined banks, are concave during the longer part of their course, but towards their termination they

* See my Travels in the Alps, 1st Edit. pp. 167, 376, and letter to Dr. Whewell in Jameson's Journal, Oct. 1844.

become convex as their viscosity increases. Nevertheless, I have seen portions of well-bounded streams decidedly convex.

The appearance of the termination of a lava stream approaches strikingly that of a glacier. But this is much more than a vague analogy, and the accounts of faithful eye-witnesses prove the resisted motion of the doughy stream to be such as I anticipated. We find it explicitly stated over and over again in the writings of Dolomieu* and Della Torre† (and more particularly by the latter), that when a lava stream meets with any obstacle in front which checks its course, or when its course is checked by its own sluggishness, the stream swells, and gains gradually in thickness by the fluid pressure from behind urging its particles *forwards* and *upwards*. So striking was this natural effect of semi-fluid pressure, that these old observers attributed it to a peculiar force developed in the lava, of the nature of "fermentation," producing intumescence, the only way by which they could account for the vertical rise of the fluid, although it was very evident that the result was only what might be expected from the nature of the lava. It was also observed that when the lava stream had thus attained a certain height, it began to move on again, the necessary result of the increased hydrostatic pressure, although attributed by the authors named to the heat developed by chemical action. The tenacity with which the idea was long adhered to, that the residual fluidity of a nearly cooled lava stream was insufficient to account for its progress, without attributing to it the qualities of a second volcanic focus, are curious proofs of how long a palpable cause may be rejected as insufficient to explain a phenomenon, and a totally imaginary one superadded‡.

I may add, that lava streams sometimes push their extremities *up hill*§; glaciers do the same.

In addition to the considerations already stated, which illustrate the viscous theory of glaciers, I am glad to avail myself of two which have reached me from independent and impartial sources.

The first is by Mr. Darwin, who in a small book on "Volcanic Islands," published about the time that I was engaged in making the preceding observations on Etna and Vesuvius, pointed out in a very clear manner the explanation which the veined structure of glaciers lends to that of volcanic rocks belonging to the Trachytic and Obsidian Series, where the lamination, instead of being obscure and rare, as it generally is in the Augitic lavas, owing perhaps to their greater fluidity, and more viscid and homogeneous texture, is the general rule. "The most probable explanation," says Mr. Darwin, "of the laminated structure of these felspathic rocks

* Papers in the Journal de Physique.

† Histoire du Vésuve. Naples, 1771, 8vo, p. 207–9, and several other places.

‡ See the view of the termination of a lava stream in Auldjo's Sketches of Vesuvius, facing p. 92. The reader may also compare the view of a grotto in the lava, in the same work, with that of the source of the Arveiron, in my Travels, p. 387.

§ Hamilton, Campi Phlegræi, folio, vol. i. p. 40, *note*.

appears to be that they have been stretched whilst flowing slowly onwards in a pasty condition, in precisely the same manner as Professor Forbes believes that the ice of moving glaciers is stretched and fissured. In both cases the zones may be compared to the finest agates; in both they extend in the direction in which the mass has flowed, and those exposed on the surface are generally vertical*."

The other illustration is contained in a communication with which I have been favoured by Mr. GORDON, Professor of Civil Engineering in Glasgow, and which has been printed in the Philosophical Magazine for March 1845, to which therefore I may refer. I need only state at present that it demonstrates, from observations on the flow of Stockholm pitch with a speed wholly insensible, and which requires some months for its accomplishment even in small masses, that a motion, of the nature of fluid motion, takes place at temperatures at which the pitch remains so hard as to be fragile throughout, and presents angular fragments with a conchoidal fracture. Mr. GORDON adds, that the resistance of the pitch to its own forward motion produces bands of differential velocity and having the *frontal dip*.

Edinburgh, February 26, 1845.

Note on the Velocity of Lava, referred to in p. 149.

The following are a few facts which I have collected on the velocity of lava. That of Vesuvius in 1805 appears to be the most fluid on record. VON BUCH, who was in company with MM. DE HUMBOLDT and GAY-LUSSAC, describes it as shooting suddenly before their eyes from top to bottom of the *cone* in one single instant†, which must correspond to a velocity of many hundred feet in a few seconds without interpreting it literally. MELOGRAMI, quoted by BREISLAK‡, says it described three miles in four minutes, or about seventy-five feet per second *at a mean*. The same lava, when it reached the level road at Torre del Greco, moved at the rate of only eighteen inches per minute, or three-tenths of an inch per second§. The lava of 1794 (Vesuvius) reached the sea, a distance of 12,961 feet, in six hours, or passed over one-third of a mile per hour, or eight inches per second‖; whilst the lava of Etna, in 1651, described sixteen miles in twenty-four hours, or above a foot per second the whole way. That of 1669 (Etna), which destroyed Catania, described the first thirteen miles of its course in twenty days, or at the rate of 162 feet per hour, but required twenty-three days for the last two miles, giving a velocity of twenty-two feet per hour¶; and we learn from DOLO-

* DARWIN on Volcanic Islands, 1844. The whole passage, pp. 65–72, illustrates this analogy.

† Bibliothèque Britanique, vol. xxx. The vertical height of the cone proper is 700 or 800 feet; the length of the slope may therefore be 1300 feet.

‡ Institutions Géologiques, iii. 142.

§ NICHOLSON's Journal, vol. xii. ‖ BREISLAK, Campanie, i. 203.

¶ FERRARA, Descr. del Etna, p. 105. This appears from the dates, though at variance with one assertion of the author.

MIEU, that this same stream moved during part of its course at the rate of 1500 feet an hour, and in others took several days to cover a few yards*.

The lava of 1753 (Vesuvius), starting with a velocity of 2500 feet per hour, soon diminished to sixty feet†, as did that of 1754 to the same‡; and of 1766 to thirty feet per hour§. The lava of 1831 (Vesuvius) moved over 3600 feet in twenty-six hours, and finally advanced steadily at the rate of ten feet an hour‖. The lava of Etna of November 1843, is said to have moved over three paces per second at the distance of a mile from the crater.

The stream of 1761 (Vesuvius), before it stopped flowing, advanced but three yards a day¶; and that of 1766, which continued moving for about nine months, moved over but a small space in that time. Had the attention of authors been equally directed to the *slow* as to the rapid advancement of lava, there is no doubt that we should find many instances besides these recorded by DOLOMIEU and SCROPE, of continuous movements of three feet, and even one foot a day, or less.

* DOLOMIEU Isles Ponces, p. 286. Note.

† DELLA TORRE, Histoire, &c., p. 196.

‡ Ibid. p. 130.

§ HAMILTON, Campi Phlegræi, i. 19.

‖ AULDJO, Sketches of Vesuvius, p. 79, with a sketch of the front of the stream whilst advancing at this rate.

¶ DELLA TORRE, p. 182.

XIII. *Illustrations of the Viscous Theory of Glacier Motion.*

Part II. *An attempt to establish by observation the Plasticity of Glacier Ice.*

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§ 3. De Saussure's *Theory*.
§ 4. *Modifications of* De Saussure's *Theory*.
§ 5. *Experiments at Chamouni on the Plasticity of Ice.*

§ 3. De Saussure's *Theory*.

When Gruner proposed the explanation of glacier motion by the sliding of the ice over its bed, and De Saussure illustrated and confirmed it by considerations drawn from the lubricating action of the earth's heat melting the ice in contact with the soil*, there is no reason to suppose that either of them thought it necessary to take into account the varying form of the channel through which the glacier had to pass, and the consequently invincible barrier presented to the passage of a rigid cake of ice through a strait or narrow aperture when it occurred. This is the more remarkable, because he conceives that the *inequalities of the bed or bottom* may be overcome by the hydrostatic pressure of the water, which he supposes may be imprisoned between the rock and the ice, so as absolutely to heave the latter over the resisting obstacles.

I believe that in no part of De Saussure's writings will there be found any, the slightest reference to the possibility of the glacier when fairly formed *moulding* itself to the inequalities of the surfaces over which gravity urges it; nor is there any trace of the correlative fact of an unequal motion of the sides and centre of the ice, which may in some sense be considered as the geometrical statement of the preceding physical fact. The fact of plasticity was suspected by Basil Hall, and more distinctly announced by Rendu, as shown in the first part of this paper; but it could not be proved until the geometrical fact of the swifter motion of the centre of the glacier relatively to the sides was established in 1842†. The contrary opinion at that time

* To do Gruner justice, he appears to have been aware of the effects of the earth's heat and the lubricating action of the water thawed from the glacier: "Lorsque les côtés de l'amas [de glace] qui touchent la montagne, fondent en entier, toute la masse entraînée par son poids glisse sur son fond et s'avance dans la vallée," French translation, p. 333 ... "il est vraisemblable que leur surface inférieure [*i. e.* des glaciers] se liquéfie autant, et peut-être plus que la supérieure," ib. p. 289.

† Edinburgh Philosophical Journal, October 1842, and Travels in the Alps of Savoy, p. 134.

generally entertained would have been conclusive against the hypothesis of plasticity called forth by the gravity of the mass.

So far, then, as appears from his writings, De Saussure considered the ice of glaciers to constitute a mass possessing rigidity in the highest degree, such rigidity in short as common experience assigns to ice tranquilly frozen in small masses, which is sensibly inflexible. It is in this sense in which I have spoken of De Saussure's sliding theory, as one which "supposes the mass of the glacier to be a rigid one sliding over its trough or bed in the manner of solid bodies*," and I adhere to the definition as excluding the introduction of the smallest flexibility or plasticity, to which the term rigidity is correctly opposed. I consider too that De Saussure's theory supposes the mass of the glacier to slide over its trough or bed in the manner of *solid* bodies, that is, not as a heap of rubbish or absolute fragments, such as a glacier sometimes precipitates over a rock, but which evidently did not enter into De Saussure's explanation, nor, in fact, required any theory.

As to the crevasses which form so prominent a feature of many glaciers (although many are in parts almost devoid of them), I do not recollect that De Saussure alludes to them as *facilitating* in any way the movement of the glacier, but simply as *results* of its motion and of the rigid character of ice. And I believe that this view (whether it was held by De Saussure or not) is substantially correct. The regular system of crevasses of a glacier is approximately transverse, rather arched upwards towards the origin of the glacier, and as De Saussure supposes the glacier to be pressed downwards by the mass of snow accumulating at its head, it is hard to believe that he could have regarded these fissures as in any way essential to its movement, even were they very numerous; the tendency of such a pressure from above would rather seem to be to pack the ice like an arch, opposing its convex side to the direction of the pressure.

The view now given of De Saussure's theory of glacier motion is not only conformable to what may be gathered from his writings, but expresses the unanimous understanding of his numerous commentators, followers and opponents. As some doubt has lately been hinted as to the definiteness of De Saussure's conception of a glacier as a mass devoid of flexibility and plasticity and urged down a slope *as a whole*, by the lubricating action of fusion in contact with the soil to an extent which, in extreme cases, might even give it the character of buoyancy, I will take the liberty of quoting some indisputable authorities amongst writers of name in different countries.

And first from De Saussure himself:—

"La chaleur de la terre fait fondre les neiges et les glaces, même pendant les froids les plus rigoureux lorsque leur épaisseur est assez grande pour préserver du froid exterieur les fonds sur lesquels elles réposent."—*Voyages*, § 532. *** "C'est elle qui entretient les torrents, qui, même pendant les plus grands froids, ne discontinuent jamais de sortir de tous les grands glaciers." § 533. ***

* Travels, p. 362.

"Presque tous les glaciers reposent sur des fonds inclinés; et tous ceux d'une grandeur un peu considérable ont au-dessous d'eux, même en hiver, des courans d'eau qui coulent entre la glace et le fond qui la porte. On comprend donc que ces masses glacées, entraînée par la pente du fond sur lequel elles reposent, dégagées par les eaux de la liaison qu'elles pourraient contracter avec ce même fond, soulevées même quelquefois par ces eaux, doivent peu à peu glisser et déscendre en suivant la pente des vallées, ou des croupes qu'elles couvrent." § 535.

"Quand on considère que ces glaces reposent sur des plans inclinés, qu'il coule sous elles des torrens d'eaux qui les fondent par en bas, les détachent et les soulevent, ne sent-on pas que leur permanence dans la même place est une chose *physiquement impossible*?" § 2284.

RAMOND's account:

"La cause [de la marche des glaciers] est facile à concevoir: une masse qui pèse sur un plan incliné tend nécessairement à déscendre, et cette tendance est favorisée dans les glaciers par le choc des torrents qui roulent sous leur voûtes, par l'humidité que leur masse communique au terrain qui les porte, enfin par cette multitude innombrable de cavités qui creusent leur partie inférieure, et dont l'effet est de diminuer le frottement en diminuant l'étendue des surfaces*."

ELIE DE BEAUMONT's account of DE SAUSSURE's theory: speaking of the lava of Etna, he says, "L'écorce supérieure d'une coulée separée de l'écorce inférieure et du sol sousjacent par une certain épaisseur de lave liquide, ou du moins visqueuse, se trouve dans un état comparable à celui d'un glacier, qui, ne pouvant adhérer au sol sousjacent à cause de la fusion continuelle de la couche inférieure se trouve contraint à glisser†."

BISCHOFF's account:

"Das jährliche Vorrücken der Gletscher welches SAUSSURE ganz einfach aus einem allmäligen Herunterrutschen der unteren durch das Aufthauen des Eises schlüpfrig gewordenen Seite des Gletschers auf der schiefen Fläche des Bodens erklärt, ist eine bekannte Thatsache‡."

AGASSIZ's account:

"Autrefois on admettait tout simplement qu'ils glissaient sur leur fond, en vertu de leur propre pesanteur, et que ce glissement était favorisé par les eaux au fond de leur lit. C'était l'opinion de SAUSSURE§."

MARTINS' account:

"DE SAUSSURE, ESCHER DE LA LINTH, ANDRÉ DE LUC, attribuent cette progression au poids des glaces et à l'affaissement produit par la fonte de la face inférieure qui repose sur le sol‖."

* Voyages en Suisse par COXE, traduit par RAMOND, ii. 119, 1790.

† Mémoires pour servir à la Déscription Géologique de la France, iv. 177, published 1838.

‡ Wärmelehre, p. 180, published 1837. § Etudes sur les Glaciers, p. 152, published 1840.

‖ Sur les Glaciers de Spitzberg. Bibliothèque Universelle, 1840, tom. xxviii. p. 166.

STUDER's account:

"Die bisher fast allgemein herrschende Theorie erklärte die Bewegung der Gletscher aus der Schwere allein. Es soll die Gletschermasse als STARRER KÖRPER auf ihrer Felsgrundlage, wie auf einer schiefen Ebene, theils durch ihr eigenes Gewicht theils durch dem Druck der höheren Eis- und Firnmasse herunter gleiten (GRUNER, RAMOND, KUHN, DE SAUSSURE, ESCHER)*."

This last testimony of the most exact and most learned of the living Swiss geologists as to the sense in which DE SAUSSURE's theory has always been understood is so important that I shall add a translation: "The hitherto generally prevailing theory explains the movement of the glaciers by gravity alone. The glacier masses are considered as RIGID BODIES, which slide down over their rocky beds partly by their own weight, partly under the pressure of the higher ice and *névé*." My interpretation of the views of DE SAUSSURE as regards the rigidity of the glacier ice is thus borne out by an independent authority, for M. STUDER's work and my own appeared simultaneously. It is further confirmed by private communication with another eminent Swiss naturalist nearly connected by relationship with DE SAUSSURE himself, who is more intimately acquainted with the opinions and writings of his illustrious kinsman than any other person now alive, and who considers that DE SAUSSURE's views were confined to the general analogy of the glaciers to solid masses sliding down inclined planes, and that the effects of the inequalities of the channels and forms of the ice-basins were not comprehended in his theory.

If we feel surprise that a naturalist and observer so eminent had not adverted to the difficulty of imagining a solid cake of ice, even though perfectly detached from its bed, to disengage itself from the obstacles and sinuosities of its rocky channel, we should remember,—*first*, that the explanation is given in the most general terms, and there is no appearance that its author looked more closely at its consequences and details than to satisfy himself that a sliding motion in the abstract was rendered possible by the action of the earth's proper heat, an ingenious and philosophical element of the theory (however inadequate), and that which being due principally to DE SAUSSURE, renders the theory properly his, and connected it with his ingenious inquiry into this curious part of physics as a distinct and wholly independent investigation. *Secondly*. Every one knows how an application of a principle so true and so ingenious leads men of even the most exact habits of thought to overlook difficulties in a subject almost unstudied. DE SAUSSURE did much for our knowledge of glaciers, and he saw much which no one had observed before him: we must not blame him if, yielding to a true and natural analogy of sliding bodies, he overlooked real and great difficulties inherent in the conception of a glacier as a solid continuous mass and highly rigid. *Thirdly*. In DE SAUSSURE's time no plan or map, worthy of the name, of any glacier existed, and this was a blank which even DE SAUSSURE did not attempt to supply. The popular notion of a glacier, which it is certain he had in his mind

* Lehrbuch der physikalischen Geographie und Geologie, 1844.

when he penned the passages which relate to their motion, is a mass of ice of small depth and considerable but uniform breadth sliding down a uniform valley, or pouring from a narrow valley into a wider one, as is the case with a vast majority of glaciers tolerably accessible, and which alone were visited at the time of publication of the first edition of the *Voyages dans les Alpes*. In all these cases the lateral resistance might easily be overlooked, and the popular comparison to one solid body sliding on another and lubricated by its own liquefaction might be accepted as a complete explanation; as has even been done at a later period by those who have attempted to illustrate DE SAUSSURE's theory by experiment, but who, like him, neglected the form and undulations of the bed in which it rests.

§ 4. *Modifications of* DE SAUSSURE's *Theory*.

DE SAUSSURE and his immediate followers appear to have considered the crevasses which occur transversely in most glaciers, as the result of the inequalities of the beds down which they are constrained to move; but other writers have imagined that the part which these crevasses perform in the phenomena of glacier motion is fundamental, and essential to the existence of the movement at all. Some writers have remarked that the fall of ice blocks over the precipice which often occurs near the lower end of glaciers, leaving the superior portions unsupported, allows them to advance to fill the position formerly occupied by the portion of the now fallen ice. But in this case it would appear that cause and effect are in some degree confounded. The ice about to be projected over the cliff must either advance towards its fall by its own gravity, or by the pressure of the parts behind. If its own gravity suffices, the same cause will urge the ice behind it to move similarly, whether the block in question fall or not; and if it be the pressure from behind which shoves it on, then still more is the pressure of the entire glacier the cause of motion of the entire glacier, irrespective of the precipitation of its more advanced part.

Thus, M. MARTINS' theory of the progression of glaciers is, that the weight of the parts causes them to separate by fissures into wedge-shaped masses, without their sliding along the bottom; that the fissures become filled with frozen snow, and that thus the glacier is perpetuated and extended year by year. "Cette progression," he says, "n'est donc ni un glissement ni un affaissement difficiles à comprendre, puisque la glace doit adhérer au sol, mais un démembrement successif*." Besides other objections, it is now universally admitted that the glacier-proper does not grow by the consolidation of snow in its fissures.

But setting aside the attempt to render the sliding motion of the entire glacier considered as a plane slab more easy, by considering the motions of the parts instead of the motion of the whole, we are led to notice the attempt to reconcile the sliding theory to recent observation, by ascribing to the crevasses of the glacier the important office of enabling it to accommodate itself to the inequalities of its channel.

* MARTINS sur les Glaciers de Spitzberg et de la Suisse. Bibl. Univ. Juillet 1840.

Our object is, in this section, merely to state the view in its most plausible form, which in the succeeding section we shall controvert by experiments giving it a direct negative. In the third portion of this essay we shall enter more at large into the phenomena of crevasses, and mention other objections to this hypothesis and every modification of it.

According to this view, the friction of the ice against the sides of the valleys will produce a dislocation of the glacier into longitudinal stripes (as shown in Plate VII. fig. 1.*), where a transverse line *bb'* becomes by the irregular motions of the ice distorted into the zigzag form *hcc'h*. Or if we suppose the plasticity of the ice to be sensible, but that its action is accompanied with fractures, the abruptness of the angles of the figure will be softened, as in the broken line *lmm'l* in the lower part of the same figure. This latter hypothesis evidently merges into the true plastic theory, when the part of the progression due to the flexure of the transverse lines bears a large proportion to the effect of the longitudinal slide, or more generally, when the surfaces of sliding or yielding become greatly multiplied, when the notched line will merge into a curve.

The passage of the glacier through a gorge or contraction is explained on the same view by figure 2, where the resistance of the sides having occasioned a series of parallel longitudinal rents as before, the portion of the glacier beyond the limits of breadth of the gorge BB' is supposed to be detained or embayed whilst the intermediate columns slip through.

§ 5. *Experiments at Chamouni on the Plasticity of Ice.*

It has been shown that in order to reconcile De Saussure's theory of sliding motion with the ascertained fact that the centre of the glacier moves faster than the sides, it had been assumed that solutions of continuity or longitudinal crevasses were formed parallel to the length of the glacier, by means of which the central portion slides past that adjacent to it, and so on for successive strips as we approach the sides, the more rapid retardation near the sides being rendered mechanically possible by the increased number of these longitudinal dislocations.

The result was therefore predicted to be that the glacier would be found to move by *echelons*, or that strips of ice of a certain number of feet, or yards, or fathoms, would move either suddenly or by gradual sliding, but at all events so as to mark by an abrupt separation at the longitudinal fissure, that the one portion of ice has slipped past the other by a distinct measurable quantity.

When I first learnt at Geneva, in August 1844, from Mr. Hopkins's published papers†, that this was really the author's meaning, it occurred to me that the proof

* These figures and their interpretation are taken from Mr. Hopkins's First Memoir in the Cambridge Transactions, vol. viii. part 1. A figure similar to the first is to be found in a more recent paper by the same author in the Philosophical Magazine for June 1845.

† Cambridge Transactions, vol. viii.

between the rival theories was easy, and that it was only necessary to place a series of marks in a right line transversely to the glacier, and observe whether they were displaced by an imperceptible flexure, or whether they slid past one another by sudden dislocations.

Such a proof was independent of any assertion as to the existence or not of such fissures as those contended for, about which different opinions might be formed, especially as they might be asserted to exist although invisible to the eye. Being satisfied in my own mind of the non-existence of such fissures wherever the ice is not violently dislocated and descends a steep place in a tumultuous manner (which, as already mentioned, is not the case which we consider), I had no hesitation in predicting that the result of the experiment would be confirmatory of my theory, and contradictory of the other; that the transverse line would be found to become a continuous curve, and that no other system of fissures could be found in the glacier satisfying the mechanical postulate of the greater velocity of the central parts of the glacier, than the *ribboned structure* of the ice, which I had already pointed out as resulting from a forced separation of the semi-rigid ice, at a vast, though finite, number of points in the breadth of the glacier, and which I showed to exist exactly in the direction required for releasing the mass from the tension induced by the gravity of its parts.

Having gone to Chamouni a few days later, I looked out for a place where the ice should be as compact as possible, wholly devoid of open fissures, and if possible continuous up to the bank. This latter condition I found it impossible to fulfil on the Mer de Glace, at least without ascending to the *névé*, which might be objected to as less rigid than the glacier proper. The former condition was well-satisfied in a sort of bay on the west side of the Mer de Glace between the Angle and Trelaporte, exactly under the little glacier of Charmoz. The part adjoining the western shore of the glacier is indeed highly crevassed, and therefore unfit for this experiment; but at the distance of fifty or sixty yards from the moraine it becomes remarkably flat and compact for a space of about seventy yards in width, and several hundred yards in length, throughout which space there is not a single open crevasse. Now this compact area of ice presents the veined structure in a nearly longitudinal direction, with a degree of delicacy and distinctness not to be found in any other part of this glacier (as I had already remarked in my *Travels*, p. 159), and it contains no other trace of a system of longitudinal fissures or lines of separation of any kind, which could render mechanically possible the distortion of this flat compact surface of so great an extent. Now I have always observed that the veined structure near the side of a glacier is best developed where the ice is least crevassed, or the continuity of the mass most perfect; a fact stated and referred to its true cause from the first date of my speculations on the origin of the blue veins, in the following words:—"The veined structure invariably tends to disappear when a glacier becomes so crevassed as to lose horizontal cohesion, as when it is divided into pyramidal masses. Now this immediately follows from our theory;

for as soon as lateral cohesion is destroyed, any determinate inequality of motion ceases, each mass moves singly, and the structure disappears very gradually*." Now the ice at the point in question is the compacted ice which has just passed round the great promontory of Trelaporte, having been rent by numberless chasms, and which is consolidated by pressure in the bay in question, whilst the centre of the glacier being still on the steep is deeply crevassed. The structure of the even ice is continuously striped with a regularity comparable to that of the finest chalcedony for a distance of some hundred feet. This structure must *have been produced on the spot*, since no such perfect structure exists higher up, and if it did, it must have retained all the marks of dislocation due to the formation and reconsolidation of the fissures, which are so numerous and wide as to render the passage of the glacier quite impracticable if we follow the same strip of ice up towards the promontory of Trelaporte. Let it then be recollected that the structure is *produced here*, under our eyes, on the very spot where the experiments about to be detailed were made, and that the structure in question produced a vertical slaty cleavage so distinct, that the ice broken into hand specimens may be split parallel to it like any slaty rock, and that the fine hard laminæ projecting vertically after the glacier has been washed by rain, permitted the blade of a knife to be thrust between them to a depth of several inches, although they are rarely more than a quarter of an inch thick.

I shall now describe the actual measurements made upon the glacier in order that my method of proceeding in similar cases (when I have only published results) may be understood.

The general position of the experimental surface will be understood from the topographical sketch (Plate VIII. fig. 1.) The theodolite was planted at a fixed point on the ice Q, just within the crevassed portion, which intervened between it and the western shore of the glacier. This point of fundamental and constant reference was fixed by an exactly vertical hole pierced with an iron *jumper*, or blasting iron, one inch in diameter, and was frequently deepened in order to preserve the centre as exactly as possible in the same vertical line in the ice. The theodolite was centred over it at every observation by means of a plummet, which nearly filled the cylindrical hole and permitted an adjustment, which one day with another might be accurate to about one-tenth of an inch. No stick was placed in the hole, but when not in use it was covered by a large flat stone, which effectually prevents congelation in ordinary weather†. The adjustment of the theodolite on the ice is always a matter for patience, but I succeeded in rendering it perfectly stable when once erected by inserting the three feet in cavities in the ice, and filling them carefully with ice chips.

* Third Letter on Glaciers, Edinburgh Philosophical Journal, October 1842, and Appendix to Travels, 1st edit. p. 407.

† On one occasion this precaution having been neglected (in the case of a different mark on the ice), the hole was found completely frozen up after exposure to a day or two of severe weather in the month of August. It was however recovered by observing the beautiful stellar form of the ice-crystallization.

The theodolite, placed at Q, was pointed with its vertical wire on the well-defined angle of an erratic block Q1 on the opposite eastern bank of the glacier, above Les Echellets (see Plate VIII. fig. 2.). By causing the telescope to traverse in a vertical circle, a transverse line joining the points Q, Q1 was determined, and several stations were fixed in the compact ice eastwards of Q, at distances from it of 30, 60, and 90 English feet and subsequently at 120 and 180 feet. These were numbered in succession (1), (2), (3), (4), (5), and the permanence of their positions in the ice was secured as before by carefully driving vertical holes two feet deep, which were occasionally deepened, and covered with flat stones when not in use. As these points were in succession nearer to the centre of the glacier, they were expected to move with gradually increasing velocity in advance of the imaginary line Q, Q1 drawn across the ice.

But as the theodolite stationed on the glacier at Q must partake of its motion whilst the mark Q1 on the bank remained at rest, the visual line QQ1 would appear to revolve *towards* the origin of the glacier, and hence the relative advance of the points (1), (2), &c. would seem too rapid. To estimate the correction for this error the velocity of the glacier at Q must be determined, and also the distance QQ1. For the former purpose the following method was adopted. When an observation at station Q had been completed, by pointing the telescope on Q1 and observing the apparent advance of the points (1), (2), &c., the telescope was reversed in the Y's, or turned 180° towards the western moraine, upon which it indicated from day to day a new position, owing to the angular revolution of the line joining the fixed point Q1 and the moveable point Q. The point Q2 in the topographical sketch (Plate VIII. fig. 1) indicates the point where the visual line touched the moraine at the commencement of the observations on the 9th of August 1844. By the application of a scale or a similar method, the apparent advance of Q referred to the moraine Q2 was regularly measured. It is thus obvious that these apparent motions were *too great* (by the property of diverging lines) in the ratio of the distance Q1.....Q2, to Q1.....Q: and hence it became necessary to ascertain the position of Q2 as well as Q1. For this purpose a base-line of 300 feet was measured on the ice parallel to the length of the glacier, or perpendicular to the transverse visual line, extending from the point marked (3) to the point Q3 in fig. 3, whence by the theodolite the following bearings were taken :—

	°	′
Q1	0	0
(3)	83	6
Q2	148	0

From which we deduce the distance from

Q1 to (3) . . . 2479 feet.
(3) to Q2 . . . 640 feet.

But as (3) is 90 feet east of Q, we have

Q.....Q1=2569 feet. Q.....Q2=550 feet.

The apparent motion of Q measured on the moraine is greater than the true motion in the ratio $\frac{2569+550}{2569}$ or $\frac{6}{5}$ nearly. The actual motion of Q is readily deduced as well as the apparent rotation of the visual line QQ1. Thus during 16·75 days, the duration of the experiment, the apparent advance of Q referred to the moraine Q2 was twenty-three feet six inches, which at the distance Q1.....Q2 (3119 feet) subtends an angle of 25′ 54″, or almost exactly *one and a half minutes daily*.

The motion of Q during the interval of any two observations of the marks (1), (2), (3), &c. being thus known, the correction applicable to the apparent advancement of the said marks beyond the visual line is at once found by the proportion

Q.....Q1 : Q's motion :: Q.....(1) : E,

where E is the error of apparent position of mark (1). Thus, suppose the apparent motion on the moraine Q2 to be seventeen inches; this, reduced in the ratio of 6 : 5, gives 14·2 inches for the progress of Q. If the effect on the apparent place of a mark ninety feet from Q were required, we should have 2569 : 14·2 :: 90 : 0·50 inch.

I shall first detail the observations on the total motion of the glacier at Q, during the period to which the experiment extends, with the corrected daily motion and a memorandum of the state of the intervening weather, which accounts by its excessive variability for the remarkable variation of the progress of the glacier*.

TABLE I.

Date.	Interval.	*Apparent* motion from commencement.	*Corrected* daily rate.	Weather.
	days.	ft. inch.	inch.	
August 9. 6 P.M.	0	0 0		9. Fine.
10. 3½ P.M.	0·90	1 5	15·6	10. Some rain. 11. Some rain.
12. 2 P.M.	1·94	4 0	13·2	12. Some rain. 13. Snow storm. 14. Some rain. 15. Snow storm. 16. Wet.
17. 2½ P.M.	5·02	11 6½	14·8	17. Fine; melting snow on glacier. 18. Showery. 19. Fine; snow still on glacier. 20. Very fine. 21. Very fine.
20. 1 P.M.	2·94	16 0	15·0	22. Rain.
23. 2 P.M.	3·04	19 10	12·5	23. Showery. 24. Cold rain. 25. Fine.
26. Noon.	2·92	23 6	11·8	26. Fine. Glacier dry.

The first three marks on the ice, those placed thirty, sixty, and ninety feet nearer the centre of the glacier than Q, were fixed on the 9th of August, the mark (4) at 120 feet was planted on the 17th, and the mark (5) at 180 feet, on the 19th. The following are the observations on the *apparent* motions of these points past the transversal line through Q, as well as these relative motions corrected for the real movement of the station Q, as explained in last page. To avoid an illusory appearance of accuracy, the results are given to the nearest twentieths of an inch, which is below the possible errors of observation.

* See Travels in the Alps of Savoy, &c., p. 148.

Table II.

Table of the *Apparent* and *True* Motions of the Stations *relatively to Station Q.*

Date.	Interval.	Motion of Q.	Apparent relative motions.					True relative motions.				
			30 feet.	60 feet.	90 feet.	120 feet.	180 feet.	30 feet.	60 feet.	90 feet.	120 feet.	180 feet.
	days.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.
August 9. 6 P.M.	0·0	0·0	0·0	0·0	0·0			0·0	0·0	0·0		
10. 3½ P.M.	0·90	14·0	0·8	1·9	2·3			0·65	1·6	1·8		
12. 2 P.M.	1·94	25·6	1·7	3·1	3·7			1·4	2·5	2·8		
14. 1 P.M.	1·96	29·0	1·5	2·8	4·3	0·0		1·15	2·1	3·25	0·0	
17. Noon.....	2·96	44	2·1	4·0	6·0	7·1	0·0	1·6	2·95	4·45	5·0	0·0
19. 5 P.M.	2·21	33·2	1·7	2·7	4·4	6·2	8·5	1·4	2·1	3·25	4·75	6·15
23. 1 P.M.	3·83	48	2·5	5·8	7·7	10·3	15·0	1·95	4·7	6·05	8·15	11·75
26. Noon.....	2·96	34·6	2·1	4·6	6·5	8·0	11·0	1·7	3·6	5·3	6·4	8·6
Motion from 9th to 26th...	16·76	228·4*						9·85	19·55	26·9		
Motion from 17th to 26th...	11·96	159·8						6·65	13·35	19·05	24·3	
Motion from 19th to 26th...	9·00	115·8						5·05	10·4	14·6	19·3	26·5

The results in the preceding Table have been divided into three periods, corresponding to the unequal times of observation of the last two and the first three stations. The first line of addition includes the motion at thirty, sixty, and ninety feet for nearly seventeen days; the second sum contains the comparative results for four stations throughout twelve days, and the last line contains the entire relative motions of five stations for nine days. These results may be further analysed, as in the following Table, which exhibits the mean daily motion in inches corresponding to each point for these distinct periods, and also the ratio of the relative motion of each point to the actual motion of the glacier at Q, or the zero point, during the same interval.

Table III.

Interval in days.	Actual motion of Q, or zero point.	(1.) 30 feet.		(2.) 60 feet.		(3.) 90 feet.		(4.) 120 feet.		(5.) 180 feet.	
		Mean daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.
	inch.	inch.		inch.		inch.		inch.		inch.	
16·76	228·4	0·59	0·043	1·17	0·086	1·60	0·118				
11·96	159·8	0·56	0·042	1·12	0·084	1·59	0·119	2·03	0·152		
9·00	115·8	0·56	0·044	1·16	0·090	1·62	0·126	2·16	0·167	2·94	0·229

This Table shows, first, in a striking point of view, the regularity of action of the law by which the variable motion of the different transversal points in the glacier is governed, since the movement in the different intervals bears so near a proportion, that when estimated in terms of the actual motion of the glacier at the place, the relative motion of the parts scarcely differs by unity in the second place of decimals, and is generally much under it. Taking into account the inevitable errors of observation and the extraordinarily unfavourable circumstances of the weather, it is in the very highest degree improbable that this *law of continuity* of the *partial* motions can be ac-

* The true sum ought to be about four inches greater. The difference arises from the impossibility of estimating the correct velocities for the fractional intervals.

counted for by any casual justling or sliding of one finite portion of the ice past another, which would inevitably have left some of the points relatively at rest during some one of the many intervals of observation, and given to others evidence of a starting motion until friction had established a fresh position of repose amongst the struggling masses.

Secondly. This Table enables us to establish not only the continuity of motion of any one point, but the continuity of the relation which connects the points (1), (2), (3), &c. For instance, the relative motions of (1) being

·59 ·56 ·56,

and those of (2) being

1·17 1·12 1·16,

the ratios are

1·98 2·00 2·07.

In like manner the ratios between (3) and (2) will be found to be

1·37 1·42 1·44.

Thirdly. The flexure of the ice may be conveniently represented by a diagram, in which the several ordinates are set off corresponding to the relative spaces moved over. But to find the initial positions of the fourth and fifth marks, the proportional motion for the first period, when they were not observed, must be deduced from the comparative velocity of the period when the observations were comparable. Thus by Table II. the relative velocity of (3) to (4) during the time that they were observed together is 19·05 : 24·3; consequently whilst (3) moved over 26·9 inches (4) would have moved over 34·3 inches; the proportional motion for 16·75 days. In like manner for the mark (5) we have the simultaneous motions of (3) and (5) expressed by 14·6 inches and 26·5 inches, and hence by proportion, as before, we find

14·6 : 26·5 = 26·9 : 48·8 inches,

the relative motion of (5) in 16·75 days.

From these data the simultaneous relative motions of these six stations may be projected in a curve, or rather polygon, as shown in Plate IX. fig. 1. This is interesting, as showing very plainly, not only the regulated increase of swiftness of the glacier towards the centre, but that the *variation of the variation* is clearly brought out, indicated by a convexity in the direction of the motion, and confirming the general principle long ago announced by me, that the retardation is relatively greatest towards the side and less towards the centre. I appeal to any one conversant with the laws of mechanics in their practical application, whether the manifest continuity of such a law does not plainly include a continuity in the mutual action of the parts of the mass under experiment, and even independent of the manifest absence of great dislocations, would not establish the doctrine of a molecular yielding, or plasticity in the ice as opposed to the irregular justling of great blocks, admitting that such could exist unperceived.

The period through which this experiment extended (seventeen days) is conclusive against the idea that a small flexure could take place until the accumulated strain on the solid produced a rupture, which relieved the strain, and so forth, *per saltum*. The continuity of glacier motion in every case except that of precipitous descents or ice-falls, first proved by my experiments in 1842, is now universally admitted by those who have had any personal experience in the measurement of glacier motion, however opposed to my theoretical views*. The changes for seventeen days were connected (as has been shown) by a law of continuity established by numerous intervening observations; and the flexure or distortion of the ice amounted in this time to no less than *four feet* at the opposite ends of a line 180 feet in length. It is quite certain, from my own previous observations and those since made by M. Agassiz's directions on the glacier of the Aar, that the movement thus shown to have continued seventeen days without a *saltus* would have continued the whole season in the same manner. In fact, the *deformation* or flexure thus observed being sufficient to account for the whole excess of the central above the lateral motion, is in itself an explanation, and a proof that the explanation is adequate, and leaves nothing residual to be accounted for by *saltus*†.

I have more to add on this subject, but shall first give an account of an extension of this experiment on the actual flexure of the ice, upon so elaborate a scale as I scarcely ventured to hope would prove successful, especially as the time I could devote to watch its progress was small, and the circumstances of weather excessively unfavourable.

Having succeeded so well with the thirty feet station in the transverse line, I thought of multiplying the points of observation still further, so as to obtain a polygon of flexure more nearly approaching to a curve. This I did by making the first ninety feet of the transverse line, *i. e.* the space between Q and (3), Plate VIII. fig. 3, the subject of more immediate experiment, fixing in it forty-five stations only two feet apart. After several partial failures, which gave me, notwithstanding, encouraging results, I selected this plan. A space a foot wide and ninety feet long was cleared with hatchets and ice tools, so as to arrive at a nearly even surface of the hard delicately veined ice; and gutters were made so as to drain as far as possible the surface water from the part under experiment. The theodolite being stationed as usual over Q, and the vertical wire of the telescope describing a great circle passing through the line QQ1 transverse to the glacier, an assistant (Balmat), directed by my signals, bored

* See proofs cited in my Ninth Letter on Glaciers, Edinburgh Philosophical Journal, April 1845, and in second edition of Travels in the Alps, Appendix.

† Mr. Williamson, Fellow of Clare Hall, Cambridge, to whom I proposed this experimental test of the theory of movement by *echelons*, made a series of independent observations on the Mer de Glace, which coincided in result with what has been stated above. After a patient examination of these facts, and of others which he observed on different glaciers, I am glad to say that Mr. Williamson was led to abandon the theory of sliding columns or fragments, and to accept that of plasticity as connected with the mechanism of the veined structure which I have endeavoured to illustrate above.

a series of holes from two feet to two feet, forty-five in number, with a common carpenter's centre-bit, and as nearly as possible in the visual line. The holes, which were $\frac{5}{16}$th of an inch in diameter and about five inches deep, were immediately occupied by wooden pins prepared for the purpose. These pins were placed as nearly as possible in the visual straight line, but from the nature of the operation some errors were inevitable. The amount of these errors of position or zero of the marks was immediately determined by causing the vertical wire again to traverse the series, the assistant placing over the centre of the head of each pin in succession the zero point of a scale of inches divided both ways, and held parallel to the length of the glacier, so that (the divisions to tenths of an inch being very plainly marked, and divisible by estimation by the telescope) the fundamental position of each pin was determined, and considered as + if in advance of the transverse line (in the direction of the glacier's motion), and − if behind it (or nearer the origin of the glacier). The mere error of reading did not in any case exceed $\frac{1}{20}$th of an inch, though the uncertainty of centring of the theodolite over Q might amount to $\frac{1}{10}$th of an inch, or even more. The two marks nearest Q had their positions determined by a thread stretched from the station-pointer of the theodolite to the third mark, their distance being too small to be distinctly seen by the telescope.

The very same process, as regards the placing the zero of the scale on the head of the pin and reading off, was repeated on subsequent days, and the new readings *minus* the fundamental readings gave the *apparent* relative motion in the interval. This apparent motion had to be corrected, exactly as before explained, for the rotation of the visual line due to the translation of the fundamental point Q.

The following Table contains—(1.) the original readings on the four days of experiment, namely—

1844. August 20. 10 A.M.
August 21. 6 P.M.
August 23. 1 P.M.
August 26. 11 A.M.

(2.) The differences from the fundamental readings or total apparent displacements for each day, reckoning from the commencement. (3.) The same corrected for the rotation of the visual line from the following data:—

August.	Interval. days.	Motion of Q. inches.	Correction at dist. 90 feet. inches.
20 to 21.	1·33	16	0·56
20 to 23.	3·12	38	1·33
20 to 26.	6·08	73	2·56

TABLE IV.—Showing the Apparent and True relative motions of forty-five points two feet apart, in a line transverse to the axis of the Mer de Glace, 1844.

Mark, No.	Readings of Position.				Apparent Motion.			Corrected Motion.			Remarks.
	Aug. 20, 10 A.M.	Aug. 21, 6 P.M.	Aug. 23, 1 P.M.	Aug. 26, 11 AM.	August 20, 21.	August 20—23.	August 20—26.	August 20, 21.	August 20—23.	August 20—26.	
1.	+0·15	−0·1	−0·25	0·0	−0·25	−0·4	− 0·15	−0·25	−0·45	−0·15	Between 1 and 2 a slight fissure.
2.	+0·3	−0·5	−0·25?	− 0·2	−0·8	−0·55	− 0·5	−0·8	−0·6	−0·55	
3.	+0·15	+0·15	+0·6	+ 0·7	0·0	+0·45	+ 0·55	−0·05	+0·35	+0·4	
4.	−0·2	−0·1	+0·25	+ 0·3	+0·1	+0·45	+ 0·5	+0·05	+0·35	+0·3	
5.	−0·25	−0·35	−0·05	+ 0·3	−0·1	+0·2	+ 0·55	−0·15	+0·05	+0·25	
6.	−0·25	0·0	+0·6	+ 1·2	+0·25	+0·85	+ 1·45	+0·2	+0·65	+1·1	
7.	−0·2	+0·2	+0·9	+ 1·8	+0·4	+1·1	+ 2·0	+0·3	+0·9	+1·6	Veined structure very strong.
8.	0·0	+0·3	+1·2	+ 2·45	+0·3	+1·2	+ 2·45	+0·2	+0·95	+2·0	
9.	0·0	+0·5	+1·45	+ 2·5	+0·5	+1·45	+ 2·5	+0·4	+1·2	+2·0	Between 9 and 10 a fissure.
10.	−0·2	+0·15	+1·1	+ 2·3	+0·35	+1·3	+ 2·5	+0·25	+1·0	+1·95	
11.	−0·4	+0·25	+1·35	+ 2·7	+0·65	+1·75	+ 3·1	+0·5	+1·4	+2·45	
12.	−0·0	+0·5	+1·6	+ 2·85	+0·5	+1·6	+ 2·85	+0·35	+1·25	+2·15	
13.	−0·55	+0·15	+1·45	+ 3·3	+0·7	+2·0	+ 3·85	+0·55	+1·6	+3·1	
14.	−0·55	+0·35	+1·5	+ 3·7	+0·9	+2·05	+ 4·25	+0·75	+1·65	+3·45	
15.	−0·15	+0·6	+2·05	+ 4·15	+0·75	+2·2	+ 4·3	+0·55	+1·75	+3·45	
16.	−0·2	+0·65	+2·2	+ 4·5	+0·85	+2·4	+ 4·7	+0·65	+1·9	+3·8	
17.	−0·25	+0·6	+2·2	+ 4·15	+0·85	+2·45	+ 4·4	+0·65	+1·95	+3·45	
18.	−0·15	+0·7	+2·4	+ 4·85	+0·85	+2·55	+ 5·0	+0·65	+2·0	+4·0	
19.	−0·1	+0·95	+2·5	+ 5·35	+1·05	+2·6	+ 5·45	+0·8	+2·05	+4·4	
20.	−0·15	+1·0	+3·0	+ 5·9	+1·15	+3·15	+ 6·05	+0·9	+2·55	+4·9	Two fissures between 19–20 and 20–21.
21.	+0·25	+1·7	+3·55	+ 6·7	+1·45	+3·3	+ 6·45	+1·2	+2·7	+5·25	
22.	+0·1	+1·6	+3·5	+ 6·7	+1·5	+3·4	+ 6·6	+1·25	+2·75	+5·35	
23.	+0·3	+1·7	+4·0	+ 7·55	+1·4	+3·7	+ 7·25	+1·1	+3·0	+5·95	22–23, slight fissure very oblique.
24.	+0·2	+1·65	+4·0	+ 7·9	+1·45	+3·8	+ 7·7	+1·15	+3·1	+6·35	
25.	−0·3	+1·15	+3·5	+ 8·0	+1·45	+3·8	+ 8·3	+1·15	+3·05	+6·9	
26.	+0·35	+1·7	+4·3	+ 7·95	+1·35	+3·95	+ 7·6	+1·05	+3·2	+6·1	26–27, a slight fissure. It recrosses the
27.	+0·25	+1·75	+4·4	+ 8·45	+1·5	+4·15	+ 8·2	+1·2	+3·35	+6·65	line at 40–41.
28.	+0·25	+2·05	+4·9	+ 9·1	+1·8	+4·65	+ 8·85	+1·45	+3·8	+7·25	
29.	+0·4	+2·2	+5·15	+ 9·35	+1·8	+4·75	+ 8·95	+1·45	+3·9	+7·3	
30.	+0·15	+1·9	+4·9	+ 9·3	+1·75	+4·75	+ 9·15	+1·4	+3·85	+7·45	
31.	0·0	+2·0	+4·7	+ 9·0	+2·0	+4·7	+ 9·0	+1·65	+3·8	+7·25	
32.	+0·2	+2·3	+5·3	+ 9·7	+2·1	+5·1	+ 9·5	+1·7	+4·15	+7·7	
33.	+0·2	+2·1	+5·5	+10·0	+1·9	+5·3	+ 9·8	+1·5	+4·3	+7·95	
34.	+0·9	+2·9	+6·0	+10·95	+2·0	+5·1	+10·05	+1·6	+4·1	+8·15	
35.	+0·25	+2·2	+5·7	+10·5	+1·95	+5·45	+10·25	+1·55	+4·4	+8·25	34–35, a very slight fissure.
36.	+0·1	+2·1	+5·5	+10·15	+2·0	+5·4	+10·05	+1·55	+4·35	+8·0	
37.	−0·1	+2·05	+5·3	+10·5	+2·15	+5·4	+10·6	+1·7	+4·3	+8·5	
38.	0·0	+2·1	+5·9	+11·3	+2·1	+5·9	+11·3	+1·65	+4·75	+9·15	
39.	+0·2	+2·4	+6·35	+11·8	+2·2	+6·15	+11·6	+1·75	+5·0	+9·4	
40.	0·0	+2·3	+5·9	+11·1	+2·3	+5·9	+11·1	+1·8	+4·7	+8·85	
41.	+0·1	+2·5	+6·4	+12·15	+2·4	+6·3	+12·05	+1·9	+5·1	+9·7	40–41. See 26–27.
42.	0·0	+2·3	+6·25	+11·8	+2·3	+6·25	+11·8	+1·8	+5·0	+9·4	
43.	−0·1	+2·2	+6·15	+11·65	+2·3	+6·25	+11·75	+1·8	+4·95	+9·3	
44.	0·0	+2·4	+6·5	+12·35	+2·4	+6·5	+12·35	+1·85	+5·2	+9·85	
45.	−0·2	+2·4	+6·1	+12·25	+2·6	+6·3	+12·45	+2·05	+4·95	+9·9	

The results of these observations will be best understood by consulting the projected results in Plate IX. fig. 2, in which the advance of the marks is magnified twofold relatively to the spaces between them, which was necessary for the reduced scale of the engraving, although it has the disadvantage of increasing the deviations from symmetry which might arise from errors of observation. As those who have not attempted the actual execution of such experiments cannot be aware of the difficulties they entail, it may be just to mention that during weather so unfavourable as that which occurred

during the continuance of these experiments, nothing can be so irksome as the necessity of persevering in the face of physical obstacles, the only alternative which the necessary limitation of my stay afforded being to abandon them. I do not speak of the painful effort of conducting delicate observations for hours under a hot sun, whilst the feet are immersed in the liquid sludge of decaying snow, for I am not aware of having sacrificed the precision of a single observation to such a cause (though in the course of my glacier experience I have sometimes been compelled to abandon or discontinue observations), but it is easy to see that the success of experiments like this depended upon the absolute fixity of the marks inserted in the unstable and wasting surface of the glacier, and that the most dry and uniform condition of the ice seemed alone to promise a chance of finding the small pins in the exact positions in which they had been planted a day or two before. Instead of this, eleven out of nineteen days which I spent at Chamouni were wet, and notwithstanding the season of the year, the glacier was repeatedly covered with snow, which in melting under a succeeding fierce sun, left the surface honeycombed by infiltration and streaming with wet, so that the preservation of the holes was only effected by laboriously covering every one with large flat stones during the intervals of observation, and even this was not free from other disadvantages which it would take too long to particularize. On the whole, the uniformity of the triple curves exhibited in fig. 2 is surprising, considering the local errors to which the fixation of the pins was liable, and the smallness of the quantities sought. The first two curves, those for the 21st and 23rd August, are indeed as perfectly regular as it would be possible to expect from this kind of observation, even much more so than I had ever hoped to attain; but on the 26th the holes containing the pins were more degraded, and some manifest errors have arisen from this cause, and evidently affect only single marks, such as the twelfth and twenty-fifth mark, which singly have inclined forward or backward by the fusion of the ice. With these preliminaries as to the reasons why the irregularities of these curves should be judged with indulgence, I will state briefly the most apparent general results.

First. The flexure of the ice is proportional, almost exactly to the time elapsed in the intervals of the observations; and it is also graduated from point to point, not *staccato*, as would inevitably have been the case had the relative motion been due to the sliding of finite portions past one another, as in Plate VII. fig. 3. We perceive nothing of the kind.

Secondly. A singular peculiarity strikes the eye, which at first puzzled me, but when the cause was explained, confirms in no slight degree the accuracy of the methods employed and their fitness to reveal the minutest motion of the glacier. The curves of Plate IX. fig. 2, cut the axis, not all exactly at the same point; but on an average this point may be fixed with tolerable accuracy between the third and fourth mark, or seven feet from the fundamental station Q. *The first and second marks moved evidently slower than the point Q, or the zero,* and the progress of the third and fourth is dubious or irregular. The cause of this peculiarity was clearly ascer-

tained *on the spot* to be the existence of two crevasses belonging to the lateral system of crevasses, between which at their thinning out the station Q had been placed, under the idea that their distance was sufficiently great not to affect the motion. The position of these crevasses is shown in Plate VIII. fig. 3, by which it will be seen that they were fifty feet apart in the direction of the length of the glacier, and that a line joining their extremities passed eight feet nearer the centre of the glacier than the point Q, thus almost coinciding with the point of contrary flexure, which was evidently occasioned by a slightly superior advance of the mass of ice on which Q was placed, thus insulated in some degree between the two fissures. This enables us to transfer the origin of our curve to a point of undoubted solidity, namely, the fourth station, from which point it swells with the regularity which has been described, and which establishes the compactness of the ice experimented on in the most convincing manner.

Thirdly. The curves reckoned from the origin, thus experimentally indicated, show in a beautiful manner the convexity in the direction of the glacier motion before alluded to, which is singularly striking, considering the shortness of the space in which it is developed with nearly mathematical precision, being only about $\frac{1}{25}$th of the breadth of the glacier in this place (see ground plan, Plate VIII. fig. 1.). Even an inspection of the curves (Plate IX. fig. 2) can faintly convey the impression made upon my own mind, when upon the 26th of August I placed the theodolite for the last time over station Q, and caused the vertical wire to pass in front of the line of pins bent into the convex shape by the relative motion of six days' continuance. Thus seen in foreshortened perspective, the eye would in an instant have seized an abrupt motion or discontinuity of the line, but "the appearance of the curve they formed was beautiful; the whole line of pins was deviated from the usual line QQ1 by an angle equal to 12·45 inches, seen at a distance of ninety feet, or about 40′, and besides this, the pins lay in a beautiful and nearly continuous curve, presenting its convexity towards the valley, and decidedly without any great step or start. This was beautifully seen when I directed the vertical wire of the theodolite upon the forty-fifth pin and caused it to describe a vertical plane*. I observed however a curious fact, plainly indicated by the numerical results; the curve crossed the axis at the fourth pin, and attains its greatest convexity at the twenty-fifth†."

Fourthly. That no information might be wanting as to the precise condition of the mass of ice under experiment, I made a very minute examination of the state of the transverse line with respect to the occurrence of *flaws* in the ice. The most important of these was one which returned into itself, crossing the line towards the origin of the glacier between the twenty-sixth and twenty-seventh marks, and returning backwards between the fortieth and forty-first without extending further upwards. Such a flaw, even if devoid of cohesion, could only act by allowing the piece of ice contained between the twenty-seventh and fortieth mark to slip bodily forwards,

* An approximation to this effect will be obtained by stretching a fine thread over the figure.

† From my notes made at the time.

leaving a vacuity behind. No such slip is recorded in the observations, and no such gap or vacuity was left during the continuance of the observations for seventeen days. Nothing approaching to an open fissure occurred in any part of the space of ice under observation, and the few flaws noted (see the column of remarks in Table IV., and the indications of them by dotted lines and arrows in Plate IX. fig. 2, showing their directions) were perfectly close, and all more or less of zigzag forms, preventing the possibility of sliding. The *veined structure* was developed in every part of the section, in some parts more admirably than others, as near the seventh mark, and between the fortieth and forty-fifth. These countless blue veins may be considered as so many flaws or partial solutions of continuity, existing or having existed; they are almost perfectly *straight*, and (as will be shown immediately) *exactly* in the direction in which the relative motion of the parts of the ice demonstrated by these experiments takes place. It would require strong evidence to convince us that these veins are not occasioned by, and the mechanism of, the plastic motion of the ice.

The beautiful convexity of the curves in the direction of motion which the eye at once seized, and could more accurately distinguish by the theodolite, as having its largest sagitta at the twenty-fifth mark, namely, almost exactly midway between the fourth mark or convex origin, and the forty-fifth or extreme mark, contains in itself an evidence with which no person of correct habits of thought can fail to be struck as proving a regular plastic action of gravity or other propelling force acting from point to point on the mass of the glacier. I made however a check experiment almost unnecessary, but which I will here detail. Lest it should be alleged that the whole area under experiment was a moving one capable of being *swung* round by the pressure upon the centre of the glacier, so that the displacement of the transverse line was due to rotation of the mass operated on round some distant centre, I took care near the commencement of my experiment to fix a mark in the ice in the same line with Q parallel to the length of the glacier, or perpendicular to the transverse visual line at the commencement of the experiment. This point is marked q in Plate VIII. fig. 3. Now had the block of ice on which the marks Q, q, (1), (2), (3), &c. were fixed, been revolving round some fixed or moveable centre, the right angle (3) Q q would have remained a right angle, and so for (2) Q q and (1) Q q. But these angles constantly became more obtuse, and that in different degrees depending on the convexity of the curve Q, (1), (2), (3): and so of (4) and (5). It is impossible, therefore, to avoid the conclusion, that the solid ice was itself *distorted* to the amount of the excess of progression of the more central above the lateral stations.

The results are contained in the following Table, the five stations being those formerly mentioned at 30, 60, 90, 120, and 180 feet from Q. The first three stations were fixed when the visual line had an azimuth of 89° 55′ from q; the fourth was fixed on the 14th of August, when the visual line had revolved through 3′ (its daily progression towards q in consequence of the translation of the station Q being 1′ 30″ exactly), and the fifth on the 17th, when the visual line was in azimuth 89° 47½′.

TABLE V.—Showing the variable azimuths (observed) of the transverse stations with the longitudinal direction Qq.

Date.	(1).	(2).	(3).	(4).	(5).	Angle Q1 Q q.
	° ′	° ′	° ′	° ′	° ′	° ′
August 12. 2 P.M.	89 55	89 55	89 55	.....	.....	89 55
14. 1 P.M.	90 6½	90 5	90 5	89 52		
17. Noon.	90 26	90 24	90 23	90 8	89 47½	
19. 5 P.M.	90 47	90 46	90 44	90 10	89 52	
21. 6 P.M.	90 56	90 56	90 53	90 21	90 3	
23. 1 P.M.	91 9½	91 9½	91 4½	90 34	90 14	
26. Noon.	91 24	91 23	91 18	90 45	90 24½	89 28½

In the last column is the observed azimuth of the visual line joining the fixed point Q1 with the moveable station Q, from the mark q in the glacier, on the first and last observation. This angle had therefore diminished by $26'\frac{1}{2}$. Now from the known progression of Q, and the known distance QQ1, we computed at page 166 the rotation of the visual line, and found it to be $25'\ 54''$. This close correspondence is highly satisfactory, as showing that the relative positions of Q and q remained unchanged during the continuance of the experiment with reference to the imaginary transverse line by which they were adjusted. It is worthy of notice that the line Qq was exactly in the direction of the veined structure.

Edinburgh, July 1845.

XIV. *Illustrations of the Viscous Theory of Glacier Motion.*—Part III.

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§ 6. *On the Motion of Glaciers of the Second Order.*
§ 7. *On the Annual Motion of Glaciers, and on the Influence of Seasons.*
§ 8. *Summary of the Evidence adduced in favour of the Theory.*

§ 6. *On the Motion of Glaciers of the Second Order.*

Up to the year 1844 no attempt had been made, so far as I am aware, to measure the rate of motion of those comparatively small isolated glacial masses reposing in the cavities of high mountains, or on *cols*, called by De Saussure *Glaciers of the Second Order.*

Some observations had indeed been made upon a glacier of this description in 1841, and by MM. Martins and Bravais, during a residence on the Faulhorn. But it was not at that time known that the motion of glaciers was a continuous and regular one, admitting of rigorous measurement even in short intervals of time, and the importance of such observations was overlooked. They accordingly believed that the glacier in question had no sensible motion, and probably they did not attempt to observe it until a subsequent year. It is impossible now to doubt that the *Blau Gletscher*, near the Faulhorn, has a movement like all other bodies of the kind.

In July 1844, I had an opportunity of passing some days at the hospice of the Simplon, in the neighbourhood of which exists a small glacier of the second order, easy of access, and very fit for the experiment which I proposed to myself upon such bodies. Its diminutive size made it all the more suitable; for should it be found to possess a regular motion, we are certain that the *mechanism of a glacier* is continued within the small compass of a mass which may be conveniently examined in detail in all its parts. It is lodged in a niche of the mountain called the Schönhorn*, immediately behind the Simplon hospice: we shall therefore call it the glacier of the Schönhorn. From its inconsiderable extent, it might easily be overlooked by a passing traveller amidst the multitude of vast and striking objects by which he is surrounded†. It is perched, as has been said, in a kind of niche on the northern

* Also called Hübschhorn, an equivalent epithet.

† The reader will not for a moment imagine that it is the Kaltwasser glacier of which we speak, which lies also in the neighbourhood of the Schönhorn, descending from the Monte Leone and Wasenhorn, and from which the *Galerie du Glacier* on the Simplon road takes its name.

face of the Schönhorn, somewhat about an hour's steep climb above the hospice; consequently about 1400 feet higher. The hospice is itself 6580 English feet above the level of the sea; the mean height of the Schönhorn glacier may be taken at 8000 feet. I had not an opportunity of ascertaining it more accurately.

Plate X. figs. 1 and 2, shows a sketch of a front view of the Schönhorn taken from the opposite heights, and a ground plan of the glacier. The latter is sketched merely by the eye, but the scale is furnished by some actual measures. I first visited the glacier on the 20th of July 1844. It was then covered over, by far the greater part of its extent, with snow, as shown in the plan. This snow is in great part manifestly permanent, and the glacier is therefore in the state of *névé*. The general slope is from top to bottom of the plan, and its inclination is variable, depending upon the direction of the avalanches by which it is fed, of which the principal descends the rapid *couloir* marked C, when the inclination is about 35°. This avalanche forms a sort of ridge down the glacier, as indicated by the shading of the map, leaving a considerable space comparatively flat to the eastward. On the west, the snow thins off from the ridge until it exposes the ice near the part marked B, where the slope is still considerable, being 20°, and here we have the real mass of the glacier exposed, although the ice is not of an exceedingly hard or crystalline character. The front or lower termination of the glacier all along presents a steep, nearly precipitous surface of ice, sloping from 45° to 60°. This ice rests on a bed of debris of rock which appears to be inclined about 25°. Except near the precipitous termination of the glacier, there are no apparent crevasses. The surface is uniform and uninterrupted. Some water issues from beneath the steepest part of the ice; but even in the middle of the day, near the end of July, there was exceedingly little. The length, if it may be so termed, of the glacier, from back to front is about 1000 feet, and its greatest breadth 1300 feet. Its surface may be roughly estimated at twenty-six acres.

The rock of which the Schönhorn is composed, is an alternation of the slaty rocks resembling gneiss with talc slate, which are so common in this part of the Alps. To my great surprise, on one of my visits, I heard the sound of hammers and blasting in this elevated and remote spot; and found two men employed in quarrying Pot-stone (*Lapis ollaris*) for building ovens, from a retired nook beyond the glacier; the quarry is marked on the plan at E.

On the 20th of July 1844, I ascended to the glacier, accompanied by M. Alt, one of the clerical members of the Simplon establishment, and an assistant; and I fixed upon a position, marked St. on the rock on the east side of the glacier, for planting the instrument, which was then directed, as nearly as I could judge, in a line transverse to the prevailing slope of the glacier, and the telescope was made to describe a vertical plane. It was then sighted upon a well-marked quartz vein on the rock on the distant side of the glacier, marked D, by which it could at any time be brought into precisely the same position; the position of the instrument itself being

referred to a mark cut on the rock where it stood. Two marks were then fixed on the glacier; one was a pole stuck in at A, several feet into the snow of the avalanche already described as traversing the length of the glacier. The slope of the snow at the point A was about 10°; and the distance of A from the station St., by an approximate measurement, 340 feet. 350 feet further in the same direction, a hole was made with a blasting iron into the solid ice at B, where the inclination was 20°. The precise position of these marks being determined relatively to the visual line, the observation was finished at 4 o'clock P.M.

On the 23rd of July we returned. The mark A in the snow (which was so firmly driven in that it could not be withdrawn without breaking the pole) had advanced in the direction of the slope exactly four inches at 1 P.M., or in sixty-nine hours; whilst the mark B in the ice had advanced $5\frac{1}{4}$ inches in the same time; whence we have

Velocity of A in twenty-four hours	1·4 inch.
Velocity of B in twenty-four hours	1·8 inch.

The result was what I had anticipated, although it must be confessed it might be expected to be nearly the same upon any theory of glacier motion yet proposed. The slope of a glacier, *per se*, is not an index of what should be the velocity of motion on the viscous theory. No doubt, other things being equal, the velocity will be proportional to some function of the declivity, and such we have seen to be fully borne out by experiments on the Mer de Glace of Chamouni; and in the present case, the velocity under a slope of 20° was about one-third greater than that under a slope of 10°. But the analogy of a river, as well as theoretical considerations, show that the slope is but one of numerous considerations; such as (1) the *mass* of the viscous body; the smaller the mass the smaller the velocity on a given slope*; (2) the state of infiltration or wetness of the glacier altering its resistance to change of form†. Without mentioning other causes, these are quite sufficient to account for the small velocity observed, when we recollect the very insignificant mass of this glacier and its dry state arising from its great elevation, its northern exposure, and even the very inclination of its bed which keeps it in a state of perfect drainage and leaves it always in a state tending to the *snowy*, rather than that of imbibition.

§ 7. *On the Annual Motion of Glaciers, and on the Influence of Seasons.*

The first estimate of the least authority on the advance of any point of a glacier from year to year, was made by Hugi on the glacier of the Aar, from 1827 to 1836. The method employed was to measure the distance of a well-marked block of stone, resting on the ice from a transverse line determined by the fixed objects on the shore. This is the only way, generally speaking, practicable upon glaciers at a distance from habitations, and where marks cannot be conveniently renewed in the ice from time to time during the whole year. The velocity of the part of the glacier imme-

* Travels in the Alps of Savoy, 2nd edit., p. 387. † Ibid. pp. 148. 371.

diately below the promontory, called the Abschwung, was found to be about 240 feet per annum, which though neither confirmed nor invalidated by the discordant measurements subsequently made by other observers on the same glacier, has at length been substantially corroborated by a professional surveyor, M. Wild, who has recently undertaken the verification at M. Agassiz's request.

After having myself observed the motion of several points of the Mer de Glace of Chamouni during the summer of 1842, I fixed the positions of two conspicuous blocks, one near Montanvert, marked D 7; and another opposite the Tacul, marked C, or the Pierre Platte (see my Map of the Mer de Glace), by means of which I hoped to ascertain the mean annual motion in succeeding years. With respect to the latter, or the Pierre Platte, I was successful; for in September 1843 I ascertained geometrically its change of position, subject however to the uncertainty of a few yards, owing to the sliding of the block from the pedestal of ice upon which it was so picturesquely poised*, a circumstance which happens once or twice in the course of every summer.

From the 17th of September 1842, to the 12th of September 1843, the advance was (in 360 days)	256·8 feet.
Or reduced to the exact year of 365 days	260·4 feet.
Mean daily motion	8·56 inches.

Again, being enabled to repeat the measures in 1844, I found the advance—

From the 12th of September 1843, to the 19th of August 1844 (342 days)	270 feet.
Proportional motion for 365 days	288·3 feet.
Mean daily motion	9·47 inches.

In the case of the block D 7, I was less fortunate. It was very near the western side of the glacier, and though not thrown up on the shore, yet the ice on which it rested got in some manner so embayed or entangled, that though its motion had been steadily watched during the winter of 1842–43 by my able assistant, Auguste Balmat, it had scarcely moved since his last observation on the 8th of June 1843, when I visited it in September of the same year. It must be presumed that it had been much retarded previously, and hence it is clearly inadmissible to infer a proportional motion for the portion of the year when it had not been observed, as I did in the Postscript at the end of the first edition of my Travels, whilst in ignorance of the then unsuspected retardation. The motion actually observed was 432 feet in 322 days, being at the rate of 483 feet per annum, or 15·88 inches per day. This is therefore undoubtedly *below* the true measure of the annual motion of the side-part of the glacier somewhat in advance of the Châlet of Montanvert (see the position of D 7 in the Map). It may at least be of some service as an *inferior limit* of the annual motion there.

In 1843 I fixed approximately the position of a block marked P, higher up the glacier than the Montanvert, and near its left bank, exactly opposite the spot called Les Ponts.

* See Frontispiece to Travels through the Alps of Savoy.

The observation, being repeated the ensuing year, gave a motion of *about* 486 feet (the nature of the observation did not admit of the same accuracy as at station C) from the 13th of September 1843 to the 9th of August 1844, or 331 days, being at the rate of

536 feet per annum,
or 17·62 inches per day.

In 1844 I made the casual discovery of one of my staves, used to mark the position of the station A at the *Angle*, a little higher up the glacier than the last, a point of which the motion had been most carefully observed during the summer of 1842 (see Travels, p. 140). This stick still bore legibly written upon it the date when it had been fixed in the ice at station A, and as the painted marks on the rock of the *Angle* were still as fresh as when they were made, I had no difficulty in finding the exact position on the glacier which this mark had in any part of the summer of 1842, and by measuring the distance to the place where it was found (which was on a spot of the ice quite unfrequented by guides or any one else), I had good reason for believing that this must be the space over which it had travelled in the mean time; although of course I do not ascribe to this observation the weight of a direct measure, yet it proves an interesting confirmation. Reckoning from the position it occupied on the 1st of September 1842, it had advanced down to the 26th of August 1844,

or in 720 days	952	feet,
or, per annum	482·5	feet.
Mean daily motion . . .	15·87	inches.

It will be seen that this result is in close agreement with that observed at station P above mentioned, which is a little further down the glacier, but about the same distance from the side; for though the motion of P is somewhat greater for 1843–44 than the mean motion of A for 1842–44, it will be seen by the comparative observations at C already referred to, that the glacier moved more rapidly in 1843–44 than in 1842–43.

But I am now enabled to present a view of the actual progress of two glaciers during every part of the year from direct observation. For these I am indebted to the intelligent and persevering zeal of my excellent guide and assistant at Chamouni, Auguste Balmat, of whose character I have had the pleasure of forming a more and more favourable estimate the longer I have been acquainted with him. To the long training of the laborious summer of 1842, when he assisted me, he adds the further experience derived from my visits in 1843 and 1844, in the latter of which especially he became familiar with the nice precautions requisite in conducting the most accurate measurements, and received instructions from me which rendered him perfectly competent to continue by himself the simpler kind of measurements which I have alone required of him. The extraordinary exertions which he used to obtain the winter motion of the block D 7, under the Montanvert, in 1842–43, have been noticed in my former publications. On one or two occasions, as I learned afterwards from himself, being unable to ascend the usual path to the Montanvert for fear of spring avalanches,

he actually clambered with a companion up the rugged ascent from the source of the Arveiron, plunging continually up to the middle in snow, for no other purpose than to make the observation which I had requested of him; and it would be unjust not to mention at the same time the admirable, because rare, generosity, with which he positively refused for himself any share of the remuneration which I pressed upon him the following summer, as some recompense for the fatigues and dangers which he had braved to obtain for me this information. With such a person, my confidence in the observations which he has since made at points much more accessible, and with the experience of some additional years, is complete. I do not mean that mistakes may not occur, or even that the measures may not be less exact than I might have taken myself; but from my knowledge of the man, I am nearly as confident in their *being faithfully reported, exactly as they were made*, as if I had done so myself.

With a view to lighten the labour as much as possible, I selected two stations on the *glacier of Bossons*, and desired Balmat to select two on the *Glacier des Bois* (the outlet of the *Mer de Glace* towards the valley of Chamouni); all these points being tolerably accessible at every season of the year.

The general method of observation was the following:—vertical holes were driven into the ice with a 4-foot blasting iron, at the points whose motion was to be determined; and these holes were renewed from time to time as the surface of the ice wasted. A staff of wood $5\frac{1}{2}$ feet long, was stuck in each, which projected sufficiently above the snow (which never appears to have exceeded $2\frac{1}{2}$ feet deep on the glacier) to make it visible at all seasons. During winter the staves were frozen into the ice, and the waste being small, the holes did not require renewal. Two marks are then made of a permanent kind on the rocks of the moraine, or two staves driven in, or a distant object on the farther side of the glacier was observed, so as to mark out sufficiently a line transverse to the glacier, the prolongation of which passes over the hole in the ice when first made; and the advance of the hole in the ice beyond this fixed visual line marks the progress of the glacier. The want of a theodolite is supplied by directing the eye past a plumb-line suspended over the fixed mark on the moraine nearest to the glacier, the eye of the observer being over the farthest mark. As the spaces moved over were in most cases considerable, an error of a few inches, or even a foot, is not important to the result. The progress was in every case determined by means of a line marked with *English* feet and inches, left by me at Chamouni on purpose.

The results were communicated to me regularly by letter at intervals of a few weeks during the whole year, and all questions asked and explanations required by me were answered by return of post.

Those who may look with suspicion upon observations made in a remote place by a peasant of the better class, though they may not partake of my security in the results from knowing the character of the individual, will, I believe, have their doubts removed by the internal evidence of this important series of observations,

which even a philosopher could not have invented, and which, it will be seen, are confirmed by data of quite another kind over which the observer could have no control, I mean the Meteorological Registers of Geneva and St. Bernard.

Table I.

First Station on the Glacier des Bois, a little way below the Chapeau, and at about *one-third* of the breadth of the glacier from its eastern bank.

						Space moved over in English feet and inches.		Daily motion.
From 1844			To 1844.			feet.	inches.	inches.
October ..	2.	10 A.M.	October	14.	9 A.M.	32	0	32·0
October ..	14.	9 A.M.	November ..	2.	8 A.M.	43	11	27·8
November..	2.	8 A.M.	November ..	19.	4 P.M.*	34	11	24·2
November..	20.	1 P.M.	December ..	4.	3 P.M.	13	10	11·8
			1845.					
December ..	4.	3 P.M.	January	7.	3 P.M.	32	8	11·5
1845.								
January....	7.	3 P.M.	February ..	18.	3 P.M.	49	2	14·0
February ..	18.	3 P.M.	March	18.	2 P.M.	39	10	17·0
March	18.	2 P.M.	April	17.	10 A.M.	42	1	16·9
April.....	17.	10 A.M.	May	17.	8 A.M.	56	3	22·5
May	17.	8 A.M.	May	31.	2 P.M.	43	11	37·0
May	31.	2 P.M.	June	19.	4 P.M.	61	11	38·4
June	19.	4 P.M.	July	4.	10 A.M.	52	0	42·3
July	4.	10 A.M.	July	18.	5 P.M.	62	0	52·1
July	18.	5 P.M.	August	6.	4 P.M.	77	6	49·0
August	6.	4 P.M.	October	8.	4 P.M.	187	8	35·7
October....	6.	9 A.M. (?)	November ..	8.	2 P.M.	100	9	36·4
November..	8.	2 P.M.	November ..	21.	1 P.M.	32	6	30·1

Table II.

Second Station, Glacier des Bois, near the lowest extremity, just behind the "Côte du Piget."

						Space moved over in English feet and inches.		Daily motion.
From 1844			To 1845.			feet.	inches.	inches.
December ..	4.	2 P.M.	January	7.	4 P.M.	8	6	3·3
1845.								
January....	7.	4 P.M.	February ..	18.	4 P.M.	8	11	2·6
February ..	18.	4 P.M.	March	18.	3 P.M.	7	0	3·0
March	18.	3 P.M.	April	17.	11 A.M.	11	7	4·6
April.....	17.	11 A.M.	May	17.	9 A.M.	18	4	7·3
May	17.	9 A.M.	May	31.	1 P.M.	10	5	8·8
May	31.	1 P.M.	June	19.	2 P.M.	13	3	8·3
June	19.	2 P.M.	July	4.	9 A.M.	14	1	11·1
July	4.	9 A.M.	July	18.	6 P.M.	17	6	14·6
July	18.	6 P.M.	August	6.	3 P.M.	18	8	11·9
August	6.	3 P.M.	October....	6.	7 A.M.	50	1	9·9
October....	6.	7 A.M.	November ..	8.	4 P.M.	27	2	9·8
November..	8.	4 P.M.	November ..	21.	11 A.M.	8	0	7·5

* There is some uncertainty about the circumstances of this observation, which from the difficulty of corresponding satisfactorily at so great a distance about minute local occurrences, I have been unable perfectly to clear up. It is probably correct as it stands.

Table III.

First Station on the Glacier des Bossons. Some way above the Plateau, where the glacier is usually crossed; on the west side, and near the moraine.

							Space moved over in English feet and inches.		Daily motion.
From 1844				To 1844.			feet.	inches.	inches.
November..	20.	Noon.	....	December ..	4.	3 P.M.	20	4	17·3
				1845.					
December ..	4.	3 P.M.	....	January	7.	1 P.M.	44	11	15·9
January....	7.	1 P.M.	....	February ..	14.	4 P.M.	46	9	13·6
February ..	17.	4 P.M.	....	March	17.	Noon.	35	10	15·4
March	17.	Noon.	....	April	16.	5 P.M.	32	7	12·9
April.....	16.	5 P.M.	....	May	17.	3 P.M.	60	0	23·3
May	17.	3 P.M.	....	May	31.	8 A.M.	49	0	42·9
May	31.	8 A.M.	....	June	19.	10 A.M.	54	2	34·1
June	19.	10 A.M.	....	July	4.	5 P.M.	53	8	42·1
July	4.	5 P.M.	....	July	21.	2 P.M.	43	1	30·6
July	21.	2 P.M.	....	August	7.	3 P.M.	40	10	28·8
August	7.	3 P.M.	....	October....	6.	Noon.	103	0	20·6
October....	6.	Noon.	....	November ..	10.	2 P.M.	56	8	19·4
November..	10.	2 P.M.	....	November ..	22.	11 A.M.	22	5	22·6

Table IV.

Second Station on the Glacier des Bossons. Near the lowest extremity of the glacier, where free from the moraine, on the western side.

							Space moved over in English feet and inches.		Daily motion.
From 1844				To 1844.			feet.	inches.	inches.
October....	2.	2 P.M.	....	October....	13.	4 P.M.	12	11	14·0
October....	13.	4 P.M.	....	October....	31.	10 A.M.	25	1	17·0
November..	20.	1 P.M.	....	December ..	4.	2 P.M.	15	4	13·1
				1845.					
December ..	4.	2 P.M.	....	January	7.	3 P.M.	36	11	13·0
1845.									
January....	7.	3 P.M.	....	February ..	19.	1 P.M.	43	1	12·0
February ..	19.	1 P.M.	....	March	17.	1 P.M.	27	10	12·8
March	17.	1 P.M.	....	April	16.	6 P.M.	25	8	10·2
April.....	16.	6 P.M.	....	May	17.	4 P.M.	49	11	19·4
May	17.	4 P.M.	....	May	31.	9 A.M.	34	6	30·2
May	31.	9 A.M.	....	June	19.	11 A.M.	44	2	27·8
June	19.	11 A.M.	....	July	4.	6 P.M.	41	2	32·3
July	4.	6 P.M.	....	July	21.	11 A.M.	37	3	26·4
July	21.	11 A.M.*	....	August	7.	4 P.M.	30	3	21·4
August	7.	4 P.M.	....	October....	6.	2 P.M.	82	7	16·5
October....	6.	2 P.M.	....	November ..	10.	4 P.M.	17	3	5·9
November..	10.	4 P.M.	....	November ..	22.	1 P.M.	7	2	7·2

These four sets of observations are projected in Plate XI. fig. 1, where the four lower zigzag curves represent the gradation of diurnal velocity by periods, according to the method adopted in projecting my own observations in my Travels, p. 141. The

* Marked 4 P.M., perhaps by mistake, but computed on that supposition.

general accordance is sufficiently manifest, and the effect of the season of the year is beautifully shown, the following being the minimum and maximum values:—

	Daily motion in inches.
Glacier des Bois, No. I., minimum in December	11·5
Glacier des Bois, No. I., maximum in July	52·1
Ratio of maximum to minimum	$4\frac{1}{2}$: 1
Glacier des Bois, No. II., minimum in January	2·6
Glacier des Bois, No. II., maximum in July	14·6
Ratio of maximum to minimum	$5\frac{1}{2}$: 1
Glacier des Bossons, No. I., minimum in March	12·9
Glacier des Bossons, No. I., maximum in May	42·9
Ratio of maximum to minimum	$3\frac{1}{2}$: 1
Glacier des Bossons, No. II., minimum in March	10·2
Glacier des Bossons, No. II., maximum in June	32·3
Ratio of maximum to minimum	$3\frac{1}{4}$: 1

From these observations we may deduce the annual motion from November 1844 to November 1845 with considerable exactness. Allowing for the fractional parts of a year, we obtain the following results, amongst which I have included a separate computation of the mean daily motion for the summer period (April—October), and the winter period (October—April).

TABLE V.

	Bois, No. I.	Bois, No. II.	Bossons, No. I.	Bossons, No. II.
	feet.	feet.	feet.	feet.
Motion for 365 days, November 1844 to November 1845....	847·5	220·8	657·8	489·1
	inches.	inches.	inches.	inches.
Mean daily motion	27·8	7·3	21·6	16·1
Mean daily motion, summer period, April to October	37·7	9·9	28·0	22·2
Mean daily motion, winter period, October to April	19·1	4·7	15·8	10·7
Ratio, summer: winter, motion.....	2·0 : 1	2·1 : 1	1·8 : 1	2·1 : 1

I. From this Table we deduce in the first place a mean annual motion far greater than has hitherto been observed, or perhaps suspected in any glacier, that of near 300 yards, or almost *one sixth* of a mile. This is on the Glacier des Bois beneath the *Chapeau*, where the inclination of the glacier is very steep, adding a new illustration of the general principle*, that in *similar* circumstances the velocity increases with the slope. To this cause may be added the high temperature of the air of the valley to which in this part of its course it is exposed; but this last cause is alone insufficient; for

II. We find that the lowest part of the same glacier immediately behind the Côte

* Travels, 2nd edit. p. 371.

du Piget, a little way above the source of the Arveiron, and therefore still deeper in the valley, has a mean velocity nearly *four times less*, arising solely from the diminished slope and volume of the glacier in that part*. Hence there must be a condensation of the ice here, a pressure *à tergo*, the quicker moving ice pressing against the slower, consolidating it, remoulding its plastic material and sealing the crevasses; and a slight examination of the state of the glacier at the points in question will show this to be the case.

III. All that has now been said with respect to the two stations on the Glacier des Bois may be repeated with only numerical differences with respect to the two stations on the Glacier des Bossons; the one set of observations confirming the other.

IV. In both glaciers the summer motion exceeds the winter motion in a greater proportion, as the station is lower, that is, exposed to more violent alternations of heat and cold; this we shall find to be general.

Before continuing our deductions, we would call attention to the close relation which may be established between the mean temperature of any portion of the year and the velocity of the glacier corresponding to it. This is done in figure 1, Plate XI., exactly in the same way as I did when comparing my observations in the summer of 1842 with the corresponding changes of temperature†. That is to say, I have projected by *periods* (corresponding to the intervals of observation on the glaciers) the mean temperatures as observed at Geneva and at the Great St. Bernard, which are regularly published in the *Bibliothèque Universelle*, the average of which (separately deduced from the mean of daily maxima and minima, and projected in the upper part of the figure) may represent not inaptly the average temperature to which the glaciers in question, and especially the middle and lower regions of them, are exposed; and further, this average possesses the advantage of being derived from data wholly unconnected with the place or parties where and by whom the observations on the motion of the glaciers were made, and therefore are free from the remotest suspicion of either in any degree influencing the other.

* This explains a circumstance which has always hitherto been a difficulty to me; the united testimony of the best-informed inhabitants, not only at Chamouni but elsewhere (as at Zermatt and at the Simplon), to the effect that during winter the lowest end of a glacier, which terminates in a valley, does not greatly protrude, nor force the snow before it. This arises in fact from the comparative smallness of the motion which the *tongue* of such a glacier appears to possess, especially in winter.

† Travels, p. 141.

Table VI.

Mean Temperatures (by periods) on the Centigrade Scale, observed at Geneva and the Great St. Bernard*.

		Geneva.		St. Bernard.		Means of Max. and Min.	
		Max.	Min.	Max.	Min.	Geneva.	St. Bernard.
1844.	Oct. 2 to Oct. 14. ..	17·37	8·57	4·16	— 1·58	12·97	1·29
	Oct. 14 to Nov. 2. ..	11·77	5·13	1·61	— 4·93	8·45	— 1·66
	Nov. 2 to Nov. 19...	11·84	3·11	1·62	— 5·66	7·47	— 2·02
	Nov. 19 to Dec. 4...	4·93	— 0·18	— 5·11	—11·25	2·27	— 8·18
	Dec. 4 to Jan. 7.....	1·45	— 2·40	— 5·70	—10·96	— 0·27	— 8·33
1845.	Jan. 7 to Feb. 18. ..	1·47	— 4·11	— 6·92	—13·86	— 1·32	—10·39
	Feb. 18 to March 18.	4·76	— 2·74	— 3·93	—12·56	1·01	— 8·24
	March 18 to April 17.	11·27	1·57	0·19	—10·13	6·42	— 4·97
	April 17 to May 17.	16·04	5·56	5·15	— 6·36	10·80	— 0·60
	May 17 to May 31...	16·83	6·91	6·04	— 4·67	11·87	0·68
	May 31 to June 19...	23·70	12·71	11·48	1·11	18·20	6·29
	June 19 to July 4. ..	22·58	12·13	9·81	0·53	17·35	5·17
	July 4 to July 18. ..	24·01	12·84	10·20	2·08	18·42	6·14
	July 18 to Aug. 6. ..	23·28	13·20	9·25	1·68	18·24	5·46
	Aug. 6 to Oct. 8.....	21·15	11·31	7·57	0·10	16·23	3·83
	Oct. 8 to Nov. 8.....	11·84	3·78	2·38	— 3·70	7·81	— 0·60
	Nov. 8 to Nov. 21...	12·54	5·86	— 1·03	— 6·06	9·20	— 3·54

A general comparison of the curves of temperature and those of glacier motion (more particularly on the Glacier des Bois) affords a proof of the justness of the principle laid down by me in 1842, that the motion of the ice "is more rapid in summer than in winter, in hot than in cold weather, and especially more rapid after rain, and less rapid in sudden frosts†;" the evidence of the connection is plainer by mere inspection than any detail could make it. But I request attention to the apparent anomalies of the curves, as affording a stronger evidence of the fidelity with which the measurements have been made, and to the truth of the plastic theory, than perhaps even the general coincidence just referred to.

If the velocity of the glacier depend upon the completeness of its infiltration with water, rendering the whole an imbibed porous mass like a sponge, it cannot depend solely on the mean temperature of any period, but also upon the *wetness* of the surface, whether derived from mild rain, from thawing snow, or from any other meteorological accident which the register of the thermometer cannot of itself indicate‡. Further, a thick coating of snow on the glacier must defend it from the excessive cold of winter just as it defends the earth and plants, and consequently the minimum of motion will not necessarily coincide with the minimum of temperature. Now to

* The last three lines have been added during printing.

† Fourth Letter on Glaciers, Edinburgh Philosophical Journal, Jan. 1843; and Appendix to Travels, 2nd edit. p. 415.

‡ "The *proportion* of velocity does not follow the *proportion* of heat, because any cause, such as the melting of a coating of snow by a sudden thaw, as in the end of September 1842, produces the same effect as a great heat would do."—Travels, 2nd edit., p. 372.

estimate these more irregular causes is not so easy; but some light is thrown upon them by a register of the weather and state of the snow, voluntarily kept for me at Chamouni by Auguste Balmat; which forms a valuable supplement to the thermometrical register of Geneva and St. Bernard. Although the daily details would take up too much space, I will endeavour to give a faithful abstract of them so far as to give a general idea of the climate of Chamouni from October 1844 to November 1845. This diary includes (at my request) occasional notes on the state of the source of the Arveiron, which are of considerable interest.

Weather at Chamouni.

1844. *October.*—A good deal of rain during the month, which on the 10th and 16th fell as snow on the hills (nine inches at Montanvert), and subsequently to the latter day the glacier at the Montanvert was not clear of snow during the winter. 14th. Source of the Arveiron diminished to *one-fourth* (of the summer volume). Ice-vault more than half closed.

November.—Till 14th much rain and snow; fine with frost after. 20th. Source of the Arveiron very low; has not shifted its usual position.

December.—Weather generally fine throughout; cold most severe from 7th to 12th.

1845. *January.*—The weather continued splendid till the 20th; greatest cold from −2° to −5° Reaumur. 19th. The vault has disappeared at the source of the Arveiron. 20th. The first snow fell which lay at Chamouni, and continued from this day, attaining a depth of $1\frac{1}{2}$ foot in February. Up to this time all the secondary heights, even the Breven and Flegère, were clear of snow, and the weather suitable for Chamois hunting. Occasional snow till the end of the month.

February.—Snow at intervals all the month. 13th. Greatest cold of the season; thermometer −15° Reaum. followed by fine weather. 20th. Snow lies $2\frac{1}{2}$ feet deep at the upper stations on the glaciers; $1\frac{1}{2}$ foot at Chamouni. The arch of the source of the Arveiron has wholly disappeared, but the water issues at the usual places as in summer. The water is reduced to a small amount and may easily be stepped across. It is *still whitish and dirty*, though less so than in summer; *except* when a change of weather is threatened (when it is as dirty as in summer)*. *Same date.* The glacier of Bossons has extended itself much. "On ne s'y reconnait presque plus." It is advancing towards the moraine of 1818; and the lower end is at least seventy feet high.

March 1st—3rd, mild, with rain; 3rd—13th, cold; 15th, *heavy rain.* Alternate rain and fine till end of the month. 27th. Not half a foot of snow lying at Chamouni. The source of the Arveiron has not opened a vault. The quantity and muddiness of the water the same as at the last report.

* This important remark proves that in the middle of winter a temporary rise of temperature of the air over the higher glacier regions (which is the precursor of bad weather) not only produces a thaw there, but finds the usual channels still open for transmitting the accumulated snow water.

April.—First week fine; second week cold with snow; changeable to the end of the month. 16th. Source of Arveiron has not much increased in water since the middle of March. In the end of April the snow first disappeared from the lower part of both glaciers.

May.—The first half of the month fine, with occasional snow; the second half changeable, with rain. 17th. The source of the Arveiron has increased three-fourths (*means probably in the ratio of four to one*) since the middle of April, and is dirty. The ice-vault is not yet formed. 26th. The Glacier des Bossons advances rapidly and is crumbling into pyramids. The end of the glacier is at least eighty-five feet high and advances considerably, particularly during the month of May; and widens greatly.

June.—A changeable and wet month; a very late season*. The snow did not entirely disappear from the Mer de Glace opposite the Montanvert till the beginning of July. 6th—7th. The vault opened at the source of the Arveiron. The quantity of water since the end of May is the usual summer supply.

July.—Commenced with warm weather. 5th. Thermometer 27° Reaum. The snow has disappeared from the ice opposite Montanvert, but some patches remain on the way to the Jardin. The Mer de Glace is much higher in level (about forty feet) than in former years, and the marks made in the rock at the *Angle* (in 1842) are all covered. The crevasses much the same as usual. The glacier of Bossons has also increased greatly, and appears to be approaching its old moraines. The register for the greater part of July has not come to hand.

August.—A very changeable rainy month. 8th or 9th. The arch at the source of the Arveiron fell in, and did not form again during the season.

September.—Also a changeable month. Rain twelve days.

October.—A very fine month. No rain mentioned after the 7th.

A careful examination of this interesting register will explain several of the apparently irregular inflections of the curves of glacier motion. Thus (to continue our general remarks, p. 186) we find

V. At the upper station on the Glacier des Bois the least velocity occurred in December, whilst at the lower station (and at both of those on the Bossons) a minimum coinciding also with that of the temperature of the air took place in January. This coincides with the important fact noted in the preceding register, that the upper part of the Mer de Glace was covered with snow from the 16th of October, which only lay in the valley of Chamouni from the 20th of January; the snow screening the ice from the extremity of the cold.

VI. The comparative march of the two glaciers bears a remarkable relation to their positions and form. In the Bossons we detect at once the sudden transitions and seemingly capricious changes of a torrent; in the Mer de Glace we have the

* It will be seen from the temperature curves that the thermometer *fell* considerably in the latter part of June, both at Geneva and St. Bernard.

stately and regulated flow of a river, in which the slighter variations are absorbed by the predominant inertia of a comparatively stable mass. Now the glacier of Bossons is, as every one who has seen it knows, a mere icy torrent, "a frozen cataract," which descends in a continuous mass from the level of the Grand Plateau of Mont Blanc to that of the Valley of Chamouni with very little impediment, with no confining bulwarks of rock, no contracting straits; and throughout this great vertical height of at least 9000 feet, the angle of descent is very steep indeed for so vast a mass. On the other hand, though the part of the Mer de Glace, called the Glacier des Bois under the Chapeau, is very steep, its "*régime*" is regulated by the supply derived from the reservoir glacier above, and, precisely as in rivers of great magnitude and length of course, and of moderate declivity, it yields sluggishly to impulsive or retarding forces which are checked and opposed by the multitude of sinuosities, the embaying of the ice in rock-bound expansions of the channel, the struggle of its passage through defiles and the enormous friction of its lower surface. Yet, lest we might attribute the irregularities of the torrential glacier to causes quite local and uncertain, we find them reflected more or less distinctly in the movements of the neighbouring one. Thus the anomalous retardation in the end of March and beginning of April appears in three stations out of four, as does that in the first half of June, showing clearly that it is not an error of observation. It appears that the thaw of the winter's snow during the month of May, saturating the pores of the glacier with water, produced (as we know that a thaw always does) a sudden and violent march, especially of the more susceptible or torrential glacier. So completely had this sudden move forced on the glacier of Bossons, encumbered by the spring avalanches and loaded with all the fragments and snow masses which had remained temporarily suspended during the winter months, that the lower part of the glacier (as we read in the memoranda to the register) advanced and widened greatly, to an extent which it had not done for many years past, and seemed to change its whole character; and in February a similar temporary increase of volume had taken place; "on ne s'y reconnait presque plus," writes Balmat; thus accounting for the particular accession of speed which appears in that month. In both cases, after the rapid march in February and in May, a reaction takes place; the material is deficient, the excessive pressure has been removed by the previous overflow, and a lull occurs in March and in June.

VII. These irregularities, such as they are, even should we fail in entirely explaining them, are at least not to be attributed entirely to errors of observation, since different observations (which it is to be recollected were sent to England in so rough a state that they required to be reduced and computed before the variations of velocity could be deduced from them) agree amongst one another, and agree with the phenomena casually noted in the Meteorological Register. They are very trifling in the movement of the Glacier des Bois, which presents a curve of remarkable regularity, giving a minimum about the end of December, and a maximum in July. The coin-

cidence with the curve of temperature is greater throughout than we could have expected, considering the important difference of circumstances which occur in autumn and in spring when the thermometer stands nearly alike, the first chill of autumn depriving the glacier of its fluid pressure more effectually than the severer cold of winter which is tempered by its snowy covering, whilst in spring the first relaxation of the bands of frost saturates the icy mass with the impetuous streams of melted snow, as effectually as the intensest heat of summer. In fact, the velocity would probably be greatest in spring, were it not that then the ice has attained its greatest consolidation by the slow but continued effect of the winter's cold penetrating its upper layers, though after all probably to no very great depth. But this is undoubtedly the reason why the minimum and maximum approach so near to one another in point of time in the torrential glacier of Bossons, and it receives an important illustration from the independent fact of the observed condition of the source of the Arveiron, which (see the Meteorological Register), though very small in February, was still whitish and dirty before a change of weather, showing that the bands of frost were not so strong as to prevent a temporary relaxation of thaw throughout the mass of the glacier even in winter; and although the *mean* temperature of the air had been rising ever since the middle of January, and the greatest cold had occurred early in February, we find that at the end of March the source of the Arveiron was still as small as in February, and that owing to the coldness of the spring it had not even increased very much till the middle of April, when it almost suddenly resumed its summer volume. Now during all this time the velocities of the glaciers underwent but little change,—some oscillations backwards and forwards,—but took no real start until the frost had given way, and the tumultuous course of the Arveiron showed that its veins were again filled with the circulating medium to which the glacier, like the organic frame, owes its moving energy.

VIII. Being curious to see how far a relation might be established between the temperature of the air and the motion of the glacier independent of the irregularly acting causes above adverted to, I projected in Plate XI. fig. 2, the motions of the several points of the glaciers in terms of the temperature of the air for the periods already mentioned. It is to be recollected, however, that the observations of the thermometer were not made on the spot, and indeed it would have been difficult to have fixed upon a spot which should represent the mean circumstances of the whole glacier. Perhaps, therefore, the average of the observations at Geneva and St. Bernard (the mean of whose elevations is 4750 English feet above the sea, and therefore between that of Montanvert and Chamouni) may represent pretty fairly the climateric conditions of the inferior parts of the Glaciers des Bois and Bossons. Now, if we examine the curves of fig. 2, we are struck with *their almost perfect flatness until zero of the centigrade scale of temperature is reached*; but, the thawing point of ice past, the velocity manifestly goes on increasing with the temperature, in a ratio which would appear to be tolerably uniform if we neglect the irregular inflections of the curves.

IX. I am unwilling to multiply deductions which every intelligent reader will draw for himself; but one more I must add. It very clearly appears that the variations of velocity due to season are greatest where the variations of temperature of the air are greatest, as in the lower valleys; but it also appears from Remark VIII., that variations of temperature below 0° centigrade, or 32° FAHRENHEIT, produce almost inappreciable changes in the rate of motion of the ice. Hence, from this circumstance alone, we should deduce that in the higher parts of the glacier (where, for example, it freezes almost every night in summer) the variations of velocity should be least, and indeed comparatively small at different seasons. This is well illustrated by comparing the summer motions of the stations D, A and C, mentioned in the first part of this section, with their annual motion, which exhibit a much slighter excess in favour of the summer period than in the lower stations which we are now discussing. The same thing was observed by M. AGASSIZ's surveyors on the glacier of the Aar, who at first saw, in this not very great inequality, an objection to my theory. On a more searching investigation, however, the objection disappears, as in their later writings they have acknowledged. Their position of observation far up on the glacier of the Aar, in a spot having a mean temperature near the freezing point if not lower, had a summer daily motion of 7·99 inches, and a mean daily motion during the whole year of 6·41 inches*. Now at station C, or the Pierre Platte, on the Mer de Glace, the mean motion for July 1842 was 10 inches, and for the whole year, 1842–43, it was 8·56 inches. It is quite evident that the motion of any point in the midst of a glacier is controlled by that of those which precede and follow it, and that it does not necessarily result, either that all must at once suffer a similar increase or diminution of speed, or that the times of maxima and minima, or even the general form of the annual curve, shall be the same. This leads to an important practical result which we shall follow out in the next section.

§ 8. *Summary of the Evidence adduced in favour of the Plastic or Viscous Theory of Glacier Motion.*

It is often difficult to obtain a calm and full hearing for any new theory or experimental investigation; not because there is any antipathy to novelty, or that experiment is undervalued, but simply because, in an age of bustle and struggle for pre-eminence, each man is so busy with his own reputation, or the means of increasing it, that he has no leisure to attend to the claims of others; to which may, perhaps, be added, that in the general diffusion of knowledge and acquirement, each reader, finding something in every course of experiment or reasoning which he knew, or thinks he knew before, is apt to run off with the chain of ideas which that one familiar link suggests, and losing patience to follow an argument of which he thinks he can, by his own penetration, anticipate the close. He sits in judgment on errors which are of his own invention, and confronts the author with arguments and opinions already

* Comptes Rendus, Dec. 9, 1844.

thrice refuted and rejected by himself. In an age when all men would be teachers and all write for the press, the lot of an attentive reader falls to few.

I am far from saying that I have been more than usually unfortunate in this respect. But having, like others, seen my opinions disfigured for want of sufficient attention to apprehend them, or the arguments by which they are supported; ignorance of first principles hinted at, and even errors of observation imputed, where it was convenient that such ignorance and such errors should be presumed; I claim the privilege of stating afresh, though very briefly, the leading opinions which I do hold, and some arguments for them, which, if not altogether new, may be placed in a new light.

My chief analogies for the illustration of glacier motion have been drawn from the motion of a river, and by that comparison it in a great measure stands or falls. Slight and partial as is our knowledge of the mechanics of imperfect fluids, the explanation which I have given is founded upon that knowledge, and it appears to me to be sufficiently precise to warrant the inference of an identity of the mechanism in the two cases;—namely, that the movement is due to the internal pressures, arising from the weight of the mass, communicated partly or principally in the manner of hydrostatic pressure throughout a body whose parts are capable of moving or being shoved over one another (by that exertion of force which Dr. Thomas Young calls *Detrusive Force**, which overcomes what is commonly called the Friction of Fluids), so that the velocities vary from point to point of the moving body, being most rapid near the surface and centre, and least so near the banks and bottom.

So viscous fluids move, so bodies (even brittle solids, such as hard-boiled pitch) possessing the ordinary properties of solid bodies often do, if sufficient time and sufficient force be allowed†; the efficiency of time being chiefly this, that a pressure insufficient to produce instant detrusion, will, sooner or later, cause the particles to slide insensibly past one another, and to form *new attachments*, so that the change of figure may be produced without positive rupture, which would reduce the solid to a heap of fragments. This change may either take place without any loss of homogeneity, or by numerous partial and minute rents not everywhere communicating, and therefore not necessarily destructive of cohesion, which may be termed a bruise.

A glacier is not a mass of fragments.—As the analogy of the glacier to a river, in which the fluid principle is greatly in defect, and the cohering or viscous principle is greatly in excess, is the theory which I maintain, it is evident that the analogy of a stream of sand, or loose materials shot from a cart, or any other comparison with an aggregate of incoherent fragments or individual masses, must be wrong if mine be right. And I feel confident, not only that such an incoherent mass could not move after the manner of a glacier, but also that attentive inspection of a glacier at once contradicts such an idea.

* Lectures, I., 135.

† See Professor Gordon's Experiment, Philosophical Magazine, March 1845.

On the *first* point, I maintain that a rugged channel, like that of a glacier, with a moderate slope, being *packed* with angular solid fragments, would speedily be choked, and that farther pressure from behind (for such a mass can only convey thrusts, not strains) would tend to wedge the fragments more tightly. Some grains of dry sand will slide easily down a plate of glass; but try to thrust it forcibly through a narrowing tube, or even a uniform one, the lower end of which rests on a surface over which the sand has poured, and your effort is vain, the tube will sooner burst; and even rocks may be blasted rather than the power of the wedge yield*. If the figure of the bed or channel be in any degree irregular, that is, have expansions and contractions, however smooth its surface, however small the sliding angle of ice upon that surface, the choking of a strait or contraction by the piling of the fragments will be as complete and effectual as if the lateral friction were excessive. Now in point of fact we have such cases as this;—a glacier 2000 yards wide (the Mer de Glace at the Tacul) issues by an orifice or strait 900 yards wide;—the glacier of Talefre, a nearly oval basin, pours out its annual overcharge by an orifice the breadth of which is but one-third of its lesser, one-sixth of its greater diameter†. On the supposition of jostling fragments, the facility of motion is increased, as the comminution is greater. The impossibility of the discharge of a fragmentary solid through a gorge by long stripes fractured parallel to its length, and constituting parallelopipedons of a certain breadth, is evident.

Crevasses.—In the *second* place, I maintain that actual inspection shows that a glacier is not the mass of fragments nor of parallelopipedons which some persons have, naturally enough at first sight, supposed it to be. In truth there is not an approach to such a condition in those glaciers which move over moderate slopes of considerable extent, which have very properly been assumed by all writers as the criterial examples of any theory; for it is not denied that portions of glaciers and glacier tributaries do sometimes fall piecemeal over precipices, each fragment descending by its separate and individual gravity, in the manner of an avalanche, although I am disposed to believe, indeed am sure, that the number of such instances is smaller than is usually imagined; and the angle requisite for such a tumultuous mode of descent is far greater than it has, perhaps, always hitherto been considered to be. To him who would form a just estimate of the mechanical constitution of a glacier—who would consider it as a whole—without *always* distracting his attention from the length and breadth of the problem by a minute attention to its lesser features,—I would earnestly recommend the frequent and attentive survey of a glacier or glaciers from a considerable elevation above their level and under varying effects of light. Had I confined myself to studying crevasses on the surface of the glacier, measuring their depths, injecting the ice with fluids and taking its temperature; useful and important as these inquiries

* See Huber-Burnand's conclusive experiments on this subject, Ann. de Chimie et de Physique, xli. 166, and Fechner's Repertorium, i. 65.

† See the Map of the Mer de Glace and its tributaries in my Travels in the Alps of Savoy.

are, (and I might almost include the fundamental and most important inquiry of all, that of ascertaining the velocity of its parts,) I should have been much longer in seizing the general truth of the individual character of a glacier, the importance of the fluid-like connection of its parts, the perfectly secondary importance or unimportance of the fissures by which it is often traversed. The traveller who winds his tortuous and sometimes perilous path amongst these crevasses, forgets, in the fatigue of his circumventions, in the wonder of his curiosity at their beauty and seemingly unfathomable depth, in the appalling steepness of their sides and the comparative insecurity of his own footing—he forgets, I say, in the midst of all these claims upon his attention, his curiosity, and his strength of mind, the comparatively large surfaces of unbroken ice over which he heedlessly walks, and the small, the very small depth at which most of the yawning crevasses which make such an impression on his imagination, dwindle into mere slits;—and when his walk is finished, he imagines that a glacier is a mere network of fissures interlacing in all directions. But let him gain a bold height above its surface, 800 to 1000 feet at least*, so that the whole may be spread somewhat like a map before him, yet not too distant to prevent his seeing the number and forms of the crevasses, and estimating their area compared to that of the unbroken ice, his opinion is first shaken and then changed. He sees in the glacier a *whole*, which, regarded as such, is merely scarred, not dissected by these fissures;—he sees a mass as capable at least of conveying strains as thrusts; of which the cohesion is no more destroyed than (to use a comparison which I long ago employed) a parchment sieve is incapable of being stretched, because it is covered with fine slits.

I am confident that this will be plain to every unprejudiced person who will make the observation which I have recommended, and I have no hesitation in stating my belief that it will be found to be fully confirmed by M. Wild's map of the glacier of the Aar, should it ever be published; I say so without having any recollection how the matter stands, although I once had an opportunity of seeing that fine work for a few minutes; and the verification of this remark, by positive measurement, will, so far as I see, be the chief result likely to flow from the patient and disinterested labour of that competent surveyor.

But if this be true in a merely *superficial* plan, how much more true would it be if we could pare off the upper stratum of the glacier, and view a horizontal section of it at a depth of a hundred feet! The depth of the crevasses has, I am persuaded, been as much exaggerated as the thickness of the ice of the glacier has been underrated. In how few cases (where a glacier does not descend tumultuously) can we

* I may mention, as the very best stations which I am acquainted with, the summit or higher slopes of the hill of Charmoz above Montanvert, Station G*, above Trelaporte, and a point directly above the Couvercle at least 1200 feet higher than the Mer de Glace, which may easily be reached from the glacier of Talefre. Other glaciers offer of course similar points, but few so advantageous; the glacier of the Aar from the Schneebighorn, the lower glacier of Grindelwald from the slopes of the Mettenberg, the glacier of the Rhone from near the Mayenwand, and that of Zermatt from the Riffelberg, are examples.

let a plumb-line down even fifty feet without grazing the sides! and to what an insignificant fissure has the gaping crevasse dwindled even at that small fraction of the glacier's thickness! Supposing the crevasse to become uniformly narrower, how soon would it be extinct!

Again, the crevasses which traverse the surface of the glacier have almost always a determinate direction or directions, of which the simplest type seems to be that of perpendicularity to the veined structure*, which, generally speaking, occasions a convexity of the lines of fissure towards the origin of the glacier. Opposite Montanvert the crevasses form two systems inclined 65° to one another, but this appears to be a casual occurrence arising from a fresh strain being imposed on the ice owing to its rigidity when the direction of the bed or trough suddenly changes, and the two-fold systems probably coexist but for a short space, one tending to close whilst the other opens. Be this as it may, unless where a glacier is falling headlong in the manner of a cascade, the crevasses do not produce any actual dislocation of its mass into blocks or fragments, since the crevasses rarely intersect even where most numerous, but almost invariably *thin out* in the solid mass, whilst another crevasse takes its origin a little to one side or other, leaving a firm connection of ice between them; and the difficulty and danger of traversing a glacier where much crevassed, does not arise from the necessity of leaping from square to square of ice, but from having to traverse these bridges of icy communication, which even there link the glacier together, and which are almost always sharp on their upper edge when the season of the year is pretty far advanced, owing to the continual dripping.

The occurrence of crevasses which cut up a glacier into square or trapezoidal blocks, is sufficiently infrequent to deserve notice. Such occur when a glacier of the second order descends over a boss of granite, or a surface convex in all directions. We have then radiating crevasses combined with concentric ones, producing a tartan-like appearance. Such may be seen in a glacier of the second order on the south side of the Aiguilles of Charmoz and Grepon, above the Glacier du Géant; and it is a very convincing proof of the *essential tenacity* of a glacier, that, with a surface so scarred and intersected, the fragments do not fall away in avalanches. This only is to be explained by the consideration that, thin as are the glaciers of the second order, the apparent dislocation is only superficial.

Were the inequality of the central and lateral movement of the glacier mass to be attributed to longitudinal fissures or discontinuities, by means of which broad stripes of ice slide past each other, we should have to demonstrate the existence of such fissures, which could not be always close unless either (1) the surfaces were mathematically adapted to slide over one another, or (2) the ice possessed sufficient plasticity to mould the surfaces to one another's asperities, in which case the plasticity would alone be sufficient without the discontinuity to explain the motion of the ice. These longitudinal fissures, cutting the common transverse fissures perpendicularly,

* Travels in the Alps, p. 171.

would divide the glacier even where most level into trapezia, and no transverse crevasse could be straight-edged but must be jagged like a saw, or cut *en échelon*. Such a phenomenon never occurs unless where a glacier is moving *torrentially*, or with great disturbance and down a steep. *There* such longitudinal fissures may occasionally be seen, but they form the exception and not the rule. It has been demonstrated by an elaborate proof in § 5, that the only trace of longitudinal discontinuity in the normal condition of the glacier is to be found in the veined structure, which, being caused by a partial discontinuity at a vast number of points, admits of an insensible deformation of the glacial mass without sudden or complete rents, or slips, or the formation of zigzag crevasses.

The existence of the great transverse crevasses, which, even in glaciers not moving torrentially, divide the surface of a glacier by rents perhaps 2000 feet long*, have been thought by some to be comparable to beams of an elastic material, supported at the two ends, and bending under their own weight forward, in the middle. Were this the case, it would scarcely modify the plastic theory as I have propounded it; because in order that such a bar of ice should conform to the known movements of the glacier, opposite the Montanvert for instance, the centre must continually gain upon the sides at the rate of 150 feet per annum at least, consequently the limit of cohesion of an elastic solid would soon be overpassed, and plasticity in the material sufficient to explain the whole motion would inevitably be admitted at last. Independently of this, it is evident, that were such a flexure essential to the motion, the lines of crevasses would be convex in the direction in which the glacier is moving instead of towards its origin.

Argument from the Equable Progression of Glaciers.—The equability of the motions of the various parts of a glacier, united as I have shown them to be by intricate relations†, must, I think, appear conclusive to every one capable of forming a just opinion on the subject, that the relative movements of the various parts of the glacier are due to the action of forces at small distances and to the antagonism of molecular cohesions and molecular strains, and not to the casual jumbling of a quantity of rude fragments. To myself, I confess that this now appears the strongest argument of all for considering the glacier as a united mass like a river, in which there is a nice equilibrium between the force of gravitation, acting by hydrostatic pressure, and the molecular resistances of the semi-solid; the degree of regularity of the law which connects the partial movements is wonderful, and I maintain that it is inexplicable except upon the viscous theory. Thus (1) the glacier moves continually, summer and winter, day and night, and never by fits or starts; for if it does—if gravitation overcomes mere friction, it occasions a shock or avalanche; (2) its mean annual motion is nearly alike from year to year; (3) the relative velocities of points widely distributed over the glacier (but exposed to similar influences of climate), change simultaneously in the same directions, often in the same proportions; thus "the variation of velocity

* Travels, p. 171, 2nd ed.

† See § 5 of this paper, pages 167 and 168.

in the breadth of a glacier is proportional to the absolute velocity at the time of the ice under experiment*." (4) The progression of velocity from the side to the centre is marked by insensible gradations†. (5) When we compare the motion of a given point of a glacier any day of one year and the same day of another, the probability is that the velocity will be exactly the same, if the season be equally hot or cold; hence, surely, a most unexpected result, which I first announced in 1842, that *a few days' observation of a glacier will enable any one to compare its mean rate of motion over its various parts and with different glaciers.* Thus, the motion of a point marked D 2 on the Mer de Glace, was in 1842, from August 1 to August 9, 16½ inches daily; from August 9 to September 16, 18 inches; now next year, 1843, *one* observation at the same point in August gave 16 inches; and in 1844, *one* observation in September gave 17½ inches. But still further, (6) the very law of flexure of the ice is the same from year to year: a series of stations across the ice at the Montanvert gave, in 1842, the following (simultaneous) relative velocities‡:—

1·000 1·332 1·356 1·367.

The same points being recovered in 1844, the relative motions were (by a single observation of the space moved over in five days)—

1·000 1·339 1·362 1·374,

ratios almost the same but slightly increasing, which corresponds with the fact mentioned above (3), that when the absolute velocities are greater, the relative velocities are so too, which was here the case, for the velocity denoted by 1·000 was a little greater in the second case than in the first.

Tensions and Thrusts.—The occurrence of open crevasses plainly indicates the existence of strains in the ice of glaciers producing disruption, at least partially. Hence some writers have precipitately inferred that the whole glacier must be in a state of tension; an uncertain inference surely in a problem of singular complexity, and one which is not warranted by a more accurate analysis. Yet for a time rival theories seemed poised on the inappropriate question, "Are glaciers in a state of internal tension or compression?" Even if the glacier moved as a mass of fragments, therefore without tension, the cohesion must first have been broken before it could be reduced into fragments. I have been inconsiderately censured for quoting, with approbation§, the observation of M. Elie de Beaumont, that a glacier appears to be rather in a state of distension than compression, whilst I adopted a hydrostatic pressure, acting from the origin as the source of motion. A careful examination of the passages in question will show that my assent to the view of M. Elie de Beaumont was limited to *portions* of the glacier, and especially to those portions most crevassed, the parts, namely, which connect the sides and centre, and which serve to drag the more sluggish, because retarded, lateral portions after the freer central part

* Travels, p. 148.

‡ Travels, 1st edit., p. 146.

† See § 5 of this paper.

§ Travels, 1st edit., pp. 178, 370; 2nd edit., p. 370 and *note*.

on which the *vis a tergo* acts with most advantage; and in a direction generally parallel to the blue bands, so far as they are due to inequalities of motion in the horizontal plane*. My earliest attempts to obtain clear views of the internal forces acting on a semi-rigid body, impelled by self-contained hydrostatic forces, convinced me how little could be founded on the completeness of any mathematical investigation of them, which in our present state of knowledge may well be considered as hopeless; and reserving to myself the not so difficult task of extricating at a future time the more important practical laws of these strains and thrusts, I very carefully avoided, in my first publication, any allusion to what might be considered as their actual distribution; a distribution varying not only from point to point of the glacier surface, but throughout its thickness, and most undoubtedly varying also for the same point at different seasons of the year, or even changing its sign, so that a tension at one season may become a thrust at another.

I had no reason to repent of this caution, from which I only departed so far in my Seventh Letter on Glaciers, published subsequently, as to deduce in an approximate manner, from elementary mechanical laws, the directions of the *surfaces of tearing* within such a mass as I had described, upon the simple supposition that the hydrostatic pressure acting uniformly, the tendency of motion of any particle will be in the direction of least resistance when all the resistances are taken into account, and that the surfaces of rupture will divide particles whose motions are dissimilar, but will not divide particles whose motions are alike. I repeat that I had no reason to repent of my abstinence from theorizing, when I found that a far better mathematician than myself, taking up the inquiry where I had left it, and after applying himself for a long time to the exclusive mechanical considerations which the viscous theory had suggested, left the subject, as I conceive, little more advanced than he had found it, and fell into some mistakes and inconsistencies, almost inseparable from this way of treating a problem which extensive observation and patient thought can alone disentangle.

Formation of Crevasses.—It has been seen in the third section of this paper, that De Saussure, and almost all his successors, have regarded the crevasses as *accidents* of glacier motion, and not essential to it; and in this view I of course concur. Nevertheless, the study of crevasses is one of considerable, though secondary interest, and is very far indeed from being completed. It requires, among other things, a very sedulous attention to the state of the glacier at various seasons, and even whilst covered with snow; and it requires further a two-fold classification of crevasses, into those which may be considered as proper to the mass of the glacier, and those which merely seam its surface.

I will first speak of the last point.

Though the formation of a crevasse betokens a local distending force, such a force cannot with any certainty be referred to the whole depth of the glacier below the

* See Philosophical Magazine, May 1845, p. 408.

point where the chasm opens. On the contrary, there is a fully greater probability that under that very spot the ice is compressed. If one cause of a crevasse be, as is universally acknowledged, a protuberance or inequality in the bed over which the ice is impelled, for the same reason that a beam, broken by means of weights, is in a state of longitudinal compression below, where its surface is concave, and of distension above, where its surface is convex, the cracks in the glacier may be due solely to this last and partial cause. Superficial crevasses may consequently be occasioned where there is no *general* distension of the mass, either (1) by the shoving of the semi-rigid glacier as a whole, over a convex declivity, or (2) from an internal turgescence arising from hydrostatic pressure, resisted by the intense friction of the anterior or more advanced parts of the glacier, which, causing the line of least resistance to be upwards and forwards, forces the pasty mass to tumefy or increase in thickness, exactly as it has been seen in § 2, p. 153, that sluggish lava streams do in a similar case. But if the tumefaction be pushed beyond the limits of plasticity of the superior and more distended portions, they must burst and assume the crevassed forms actually observed in the plastic models described in p. 144. Hence the existence of crevasses not only does not always result from a state of *general* distension in the glacier, but may arise from the precisely contrary condition of great internal compression. This argument is well illustrated by the recent observations of M. Agassiz's co-operators on the glacier of the Aar, whose observations I have elsewhere shown* to be incompatible with any other view than that of intense longitudinal compression in the mass generally, and yet the surface abounds in crevasses of the usual form and dimensions.

The manner of formation of crevasses generally, including such as may betoken a real distending force acting on any part of a glacier throughout its thickness, is not only a most curious question in itself, but suggests others which a correct theory of glacier motion can alone answer. If a crevasse once formed remain a fissure in the ice for ever after, why is the horizontal projection or ground plan of the crevasses of a canal-shaped glacier convex towards the origin of the glacier, and not protuberant in the direction of its motion, as the ascertained greater velocity of the centre would assign? Why are the crevasses for the most part vertical and not inclined forwards, or at least not notably so, on the same account? Why, if the glacier be urged downwards by a longitudinal force distending it, do not the crevasses continually widen in proportion as they are further from the origin? These questions seem incapable of a sound answer except by supposing that the crevasses are, at least in a great degree, the fresh production of every spring, and arise from the sudden start which the glacier makes when that extremity which descends into the valley begins to experience the thawing effects of returning summer. I should not wish to speak positively upon what involves a difficult if not impossible observation,—the state of the glacier with respect to crevasses whilst still under the winter's covering of snow. But the fact

* Ninth Letter on Glaciers. Appendix to Travels, 2nd edit., p. 443

of the transverse direction of the crevasses, or even their convexity towards the origin, from year to year, seems to admit of no other explanation. But besides this, I can affirm, from a careful observation of the crevasses of the Mer de Glace from June to September in one year, that the changes which they underwent were such as preclude the possibility of a crevasse of autumn being merely preserved by the snow of winter, and re-appearing afresh in spring as it had done the previous one. The thing is impossible, because the character of the crevasse is essentially altered. In order that an autumnal crevasse may become a spring crevasse it must be sealed up, annihilated, and opened again. A glance at the three sections in Plate X. fig. 3, will illustrate this. No. 1 shows crevasses freshly opened soon after the snow has quitted the surface of the ice—the edges are sharp, the sides vertical, the openings so small that they may be easily stepped across, and in other instances they are not wider than may admit the blade of a knife. No. 2 shows the crevasse opened to its widest extent by the acceleration of the motion, by the force of the sun which has altogether wasted away the side with the southern exposure, and by the copious drippings of the melting ice and mild rain. No. 3 (which as well as No. 2 is taken from a sketch on the spot, No. 1 being done from recollection) shows what I have elsewhere called the state of collapse of the glacier, which affords the most direct possible evidence of its plastic condition; for we there see, not merely the prominences worn away and blunted by the heat of summer, but subsiding into the hollows, the crevasses being choked by the yielding of their sides, and the glacier again resumes a traversable character, only that the plane surface of spring is changed into irregular undulations preparatory to a complete amalgamation of the whole glacier into one mass*.

The collapse is thus described in my Journal of 1842, written at the time, and therefore more emphatic and unbiassed than after my theoretical views had been matured and published. "1842, Sept. 16, Friday. The level [of the Mer de Glace at the 'angle'] has sunk since the 9th of August, nine feet $8\frac{1}{2}$ inches. The effect of this immense fall is abundantly evident in this part of the glacier. On my first visit this time [*i. e.* after an absence of a month], on the 10th, I was quite struck with its shrunk appearance, as I was today with the collapsed state of the crevasses. There cannot be a question but that the glacier has subsided bodily into its bed, and that the semifused pliancy of its materials causes them to recover a uniform and lower level. The crevasses are much less deep than in July and August, as at that time they were larger and more numerous than in June. They are collapsed and (opposite Trelaporte) almost soldered up; the edges all rounded and melted by the sun's heat." The phenomena here described, "the shrunk appearance," "the *semifused pliancy*," "the soldered crevasses," "the rounded edges," convey to the attentive spectator an intuitive conviction of the plasticity of ice at the thawing season, which no words can

* See Travels, p. 174; and Fourth Letter on Glaciers.

express, no mathematical symbols weave into a demonstration. I can only say that it is easier to believe than to disbelieve; and that sooner or later, it will, I doubt not, be generally admitted.

Considering the crevasses as chiefly superficial in the normal glacier (I mean that of which the inclination is not excessive), it is evident that the formation of the crevasses must depend mainly upon the configuration of the bed. Where the section of the bed parallel to the length of the glacier is convex upwards, there the tension at the surface will cause the crevasses to expand; when the bed is concave and the surface is being compressed, the crevasses tend to close. Hence the surface of the glacier descending an irregular bed may be alternately in a state of distension and compression, and the crevasses do not tend to widen indefinitely, which would be the case if the whole glacier were distended. This tendency in the crevasses to expand and contract in accordance with their position is beautifully seen in viewing the Mer de Glace from a height, as we have recommended. The steep fall opposite Trelaporte shows the expansion of the crevasses, but the comparative level opposite the little glacier of Charmoz gives it time to recover its solidity by the general closing of the crevasses under compression. The careful study of such a scene as this gives a more clear insight into the glacier phenomena than any other part of the inquiry, excepting only the measurement of velocities.

Law of Velocities.—To these velocities we now return. The varying velocities in different glaciers, at different seasons and in different parts of the same season, are all in accordance with the motions of a viscous or plastic body. They depend upon the slope, being greatest, *ceteris paribus*, when the slope is greatest; and upon the climate to which the glacier is exposed, being greatest in glaciers which descend into deep valleys, and least in those which, though very steep (such as that of the Schönhorn described in § 6), are placed in so elevated and therefore dry and cold an atmosphere as to afford in sufficient water to moisten the snowy mass or *névé*, and which are therefore endowed with very feebly hydrostatic qualities. This is demonstrated on the one hand by the extreme smallness of their motions, and on the other by the insignificant streams of water to which they give birth even in the height of summer. In any individual glacier the velocity of the parts must (on any theory) vary with the area of section through which the ice stream has to pass; but yet it may happen that the contraction of a valley, if not accompanied (as is often the case) with an increased slope, will oppose so great a resistance to the efflux of the mass, that under intense longitudinal compression its forward motion is retarded, and the condition of uniform discharge is satisfied by the accumulation of the ice in a vertical direction, the rise of the surface being necessarily accompanied with a thrust from below upwards, and a sliding of the particles over one another in that direction. This appears conclusively to be the case for a great extent of the lower part of the glacier of the Aar, as already mentioned, and affords the most direct evidence which

could be desired, that the kind of internal motion necessary for producing the *frontal dip* in the veined structure (which arises from tearing or crushing in sliding in the vertical plane*) was correctly foreseen.

The law of velocities at different points of the axis of a glacier from its origin to wards its termination, must evidently depend upon the configuration of each particular glacier. It may be constantly increasing from the origin to the extremity, it may be diminishing, or it may have alternations of increase and diminution; and upon this circumstance the frequency and magnitude of the crevasses will mainly depend. But the *régime* of the glacier, by which we mean to express the combination of circumstances determining its motion, varies from one season of the year to another, owing not only to the general influence of heat and cold, but also to the progressive communication of that influence to portions of the glacier in successive stages of elevation. Evidently the extremity nearest to the valley will receive the earliest and most violent impression of solar heat, whilst the middle and upper regions are involved in complete winter. Partial dilatations must take place in spring, partial condensations in the decline of the year; as is evident from the consideration that temperatures inferior to freezing do not sensibly affect the motion of the ice (see above, p. 192) which higher temperatures do, consequently the influence of season will be chiefly felt in those parts of the glacier where the temperature of the air seldom falls in summer to 32°, whilst the more stable motion of the higher part acts as a drag or equalizer upon the whole system. The condition of violent distension produces crevasses, that of violent compression produces the frontal dip of the veined structure, or that share of it which is due to the relative motions in a vertical plane. The longitudinal veins will result whether the axis of the glacier be distended or compressed. Hence the reason why the frontal dip is difficultly seen in all the middle region of a glacier, which like the Mer de Glace, is subject to much extension due to great and increasing declivity, and to be well seen must be sought for in the higher parts of the glacier, as above Trelaporte, at the foot of the Couvercle†, and in glaciers subject to great compression, as that of La Brenva, the glacier of the Rhone, the Aar, &c.

Ablation of the Surface.—One phenomenon is most satisfactorily explained by the variations of velocity established and illustrated in this paper. The collapsed state of the glacier after the hot summer of 1842, and the absolute lowering of its surface level by thirty feet in the space of a few months, had struck me as requiring an energy altogether extraordinary in kind and degree to restore next spring the level which had been lost, in order to allow for an equal ablation the succeeding summer; and at first I was disposed to admit so much of the dilatation theory to be true as would account for the swelling of the surface in a vertical direction by the freezing during winter of the infiltrated water‡. Further reflection convinced me however that this explanation was insufficient and also not required, and I accordingly concluded

* Seventh Letter on Glaciers. Appendix to Travels, p. 435.

† Travels, 2nd edit., p. 167.

‡ Fourth Letter. Travels, App., p. 415.

"that the main cause of the restoration of the surface is the diminished fluidity of the glacier in cold weather, which retards (as we know) the motion of all its parts, but especially of those parts which move most rapidly in summer. The disproportion of velocity throughout the length and breadth of the glacier is therefore less; the ice more pressed together and less drawn asunder; the crevasses are consolidated, while the increased friction and viscosity causes the whole to swell, and especially the inferior parts, which are most wasted*." I have nothing to add to this explanation, except that the observation of the motion throughout the whole year confirms it in every particular. The more elevated portions of the glacier, which during a large portion of the year are exposed to a mean temperature under 32°, move in a manner comparatively uniform, the lower extremities undergo great oscillations in their speed (in the ratio of four or five to one; see page 185); hence the attenuation during the summer *régime* which is owing to the drag taking place downwards in an excessive degree; but the winter's cold, equalizing in some measure the velocity everywhere, brings the plasticity into full action, fills the crevasses and swells the surface to its old level.

As it is universally admitted that the glacier proper does not grow in thickness by snowy accumulations, the important variations in its level in different years† cannot be ascribed to the severity of certain seasons increasing the mass of snow falling upon it, but rather to the prolongation of the winter cold into spring and summer, which causes the condensing or accumulating process to be in excess, and therefore the thickness of the plastic mass to accumulate beyond its due amount.

Thus we have the following phenomena, all independently observed, reconciled and explained by one hypothesis; the general convexity of the crevasses upwards, notwithstanding the excess of motion in the centre; the general verticality of the crevasses, notwithstanding the retardation of the bottom; the perfect state of the crevasses every spring succeeding their visible collapse in autumn; the ascertained velocity of different parts of the glacier, and the diversity of the annual changes which these velocities present; the seemingly opposed facts showing the glacier to be subjected to powerful tension, producing crevasses, and yet to be under a compression which produces in some places the *frontal dip*; and finally, the renewal of the level of the ice during winter, which has been lost partly by superficial melting, but as much or more so by the attenuation and collapse of the glacier during summer. These various effects of one cause, though they do not embrace all the phenomena of glaciers, certainly include a very remarkable and complicated group of facts.

* Travels, p. 386, 2nd edit.

† For instance, it has been seen from Balmat's narrative (p. 189 above), that in 1845 the glacier attained a much higher level at the *Angle* than it had done for three previous years at least, since all the marks of measurements which were cut on the rock in 1842 were concealed: and he attributes this, apparently with reason, to the extreme lateness and coldness of the spring.

Plasticity—Veined Structure.—I certainly never expected, when promulgating the viscous theory, that it would have met with so much opposition on the ground that the more familiar properties of ice are opposed to the admission of its plasticity; and that the fragility of hand specimens should be considered as conclusive against the plastic effect of most intense forces acting on the most stupendous scale upon a body placed in circumstances which subject it to a trial, beneath which the most massive constructions of the pyramid-building ages would sway, totter, and crumble. In an age when generalizations of the more obvious kinds are no longer proofs of genius and perspicacity, and when popular writers on science delight to startle their readers by showing how bodies the most dissimilar possess properties in common; in an age in which *gradations* of properties and organs have been studied with such persevering sagacity, and in which so many unexpected qualities have been discovered;—when iron is classed as a combustible, when metals are found which float on water and which catch fire on touching ice, when a pneumatic vacuum is formed and maintained in vessels five miles long, and whose sides are ripped open twenty times a day;—when, moreover, the simpler abstractions of former times are being daily overset, when no body seems to possess any one property in perfection, and all seem to possess imperfectly every quality admitting of degree; when adamant is rejected from our vocabulary, and softness means only less hardness, and the definition of a perfect fluid is as imaginary as that of a solid without weight;—when a vacuum and a plenum are alike scoffed at, and even the heavenly bodies toil through media more or less resisting; when no substance is admitted to expand uniformly by heat, when glass may be considered a conductor of electricity, and metals as imperfect insulators;—in these days, when the barriers of the categories are so completely beaten down, I had not expected to meet with so determined an opposition to the proposition that the stupendous aggregation of freezing water and thawing ice, called a glacier, subjected to the pressure of thousands of vertical feet of its own substance, might not under these circumstances possess a degree of yielding, moulding, self-adapting power, sufficient to admit of slight changes of figure in long periods of time. Still less could I have anticipated that when the plastic changes of form had been measured and compared, and calculated and mapped, and confirmed by independent observers, that we should still have had men of science appealing to the fragility of an icicle as an unanswerable argument! More philosophical surely was the appeal of the Bishop of Annecy from what we already know to what we may one day learn if willing to be taught: "Quand on agit sur un morceau de glace, qu'on le frappe, on lui trouve une rigidité qui est en opposition directe avec les apparences dont nous venons de parler. *Peut-être que les expériences faites sur de plus grandes masses donneraient d'autres résultats*.*"

* Théorie des Glaciers de la Savoie, p. 84. Quoted in my Travels, p. 367, 2nd edit. Since this paper was read, Mr. Christie, Secretary of the Royal Society, has kindly communicated to me a very striking remark upon a well-known and easily-repeated experiment. The experiment is this. If, in the course of a severe

The "ductility" is indeed not great; the compact ice even of the slowest moving glaciers bears evidence, in the veined structure, or "blue bands," to the bruise which it has received from the all-powerful strain which has acted on it. When the difference of motions is excessive, or the slope occasions the speed to be greater than permits the gradual molecular adaptation of the semi-rigid parts to one another, the masses are broken up and fall more or less tumultuously; *the strain being then removed by the dislocation, the veined or bruised structure is invariably extinguished at last.* I shall quote a series of examples of the gradation of phenomena, which I conceive to be plainly connected by a common cause.

1. In any torrential glacier, such as the Glacier des Bossons, the upper part of the glaciers of La Brenva, Allalein, or the Rhone, and many others, the fractures are so numerous that the ice descends in blocks, almost as water in a cascade often does in spray, and hence the internal strains being destroyed, no structure is developed, or if previously developed, tends to wear out*.

2. In a glacier moving torrentially, that is with frequent and considerable changes of velocity, but without being divided into blocks by intersecting crevasses, we find real internal cracks in the ice, some feet in length, and an inch or more in thickness, marked by the pure frozen water which fills these spaces in the comparatively opake whitish ice of which glaciers descending rapidly from the region of the *névé* are composed. Such are peculiarly visible in the lower and more accessible region of the glacier of Bossons†; perhaps the most instructive which can be named as showing these infiltrated cracks, which by their dimensions, direction, and in every other particular, form a true link between the longitudinal dislocations of a torrential glacier, and the perfect veined structure or bruise into which it passes by imperceptible gradations, including a perfectly regular development of the frontal dip, where we might expect it to be well shown, for the observations of page 184 show that the lowest portion of

winter, a hollow iron shell be filled with water and exposed to the frost with the fuze-hole uppermost, a portion of the water expands in freezing, so as to protrude a cylinder of ice from the fuze-hole; but if the experiment be continued, the cylinder continues to grow, inch by inch, in proportion as the central nucleus of water freezes. "In the first instance," says Mr. Christie, "a shell of ice containing water was formed, no doubt, within the iron shell, and the fuze-hole might be filled by the expansion of the water in the act of freezing; so that there may be no reason for attributing plasticity to the ice as far as this goes; but the shell of ice once formed, and the fuze-hole filled with ice, the subsequent rise of the ice must have proceeded from the ice of the interior shell being squeezed through the narrow orifice. No thawing took place during the process. Does not this show plasticity even in very small masses of ice?" I have also been lately informed, on excellent authority, that in a new work by a most eminent German mineralogist, the plastic character of ice in masses is assumed as an admitted fact. In corroboration of what has been said in the text, I may farther add, that whilst these sheets are passing through the press, I observe in the Athenæum (June 20, 1846), an account of a patent process for moulding solid tin into tubes and other utensils, in the course of which it is stated that "tin under a pressure of about twenty tons to a circular inch, will *run* according to the law of fluids."

* See Third Letter on Glaciers. Travels, Appendix, p. 407.

† See Travels, p. 181.

the glacier of Bossons moves slower than its middle portion; there is therefore a manifest longitudinal compression arising from the friction of the bed*.

3. The next stage is that of the perfect bruise or veined structure, best seen in the most united and least fissured parts of glaciers with rocky sides and moving over a moderate slope. Whatever increases lateral compression (without however necessitating dislocation), such as the union of two or more glaciers in one, tends to develope the structure more perfectly†. Such cases are well seen on several parts of the Mer de Glace, and of the glacier of Miage‡.

4. In very wide glaciers, moving with feeble velocities, the veined structure is slightly developed, except near the sides, simply because the *twist* being small the ice is hardly bruised. Nor can we wonder to find the structure at the distance of many hundred yards from the sides of a vast slow-moving glacier of this description, if developed at all, to be complex and irregular, exhibiting twists such as I have figured in my Travels, p. 164, and which are peculiarly conspicuous in the magnificent glacier of Aletsch. This circumstance finds a precise analogue in the case of a great river, such as the Rhine, or indeed in any river moving with a very slight inclination; the excess of velocity of the central above the lateral parts, not very great at any rate, is distributed over such a space that the slightest casual disturbance of the current, from an irregularity in the bottom or sinuosities of the course, produces local differences of velocity, occasioning ripples and eddies in various parts of the breadth. If these ripples and eddies, in other words, differential motions of adjacent particles, could be visibly represented by using differently coloured fluids, they would undoubtedly afford sections exhibiting undulations and contortions exactly like those which the ice presents in the cases mentioned above. We claim therefore the apparent exception as a real proof of our general rule.

5. In the *névé* proper, no true veined structure is developed; *first*, because, whilst the mass is snowy, its powdery nature yields without admitting of a fracture or bruise; *secondly*, because the true *névé* has rarely any lateral compression worth mentioning, being widely spread and not contained between steep barriers; *thirdly*, because its motion is altogether very small; *lastly*, because its extreme dryness does not afford water enough to percolate its substance and there to be frozen; when it does so, it ceases to be *névé*.

On these grounds I hope that the theory of the veined structure, so important to that of glaciers, may be considered as explaining a number of intimately connected phenomena.

* The internal rents in the lava of Zafarana referred to in § 2 of this paper, and figured in Plate IV. fig. 8, present a perfect analogy with those of the glacier of Bossons, and appear to be due to the same cause.

† Third Letter. Travels, Appendix, p. 407.

‡ See the figures of the structure of the glacier of Miage. Travels, p. 197.

"The glacier struggles between a condition of fluidity and rigidity*." "A glacier is not a mass of solid ice, but a compound of ice and water more or less yielding, according to its state of wetness or infiltration†." "The pressure communicated from one portion to the other, will not be the whole pressure of a vertical column of the material equal in height to the difference of level of the parts of the fluid considered; the consistency or mutual support of the parts opposes a certain resistance to the pressure, and prevents its indefinite transmission. * * A glacier is not coherent ice, but a granular compound of ice and water‡." "When the semifluid ice inclines to solidity during a frost, the motion is checked; if its fluidity is increased by a thaw, the motion is instantly accelerated. * * It is greater in hot weather than in cold, because the sun's heat affords water to saturate the crevasses§." Such were the terms in which within a few months after suggesting the viscous theory I expressed my opinion of the influence of the compound structure of the glacier, a mass composed, not of ice alone, but of ice including water in its countless capillaries never frozen‖ even in winter. The quality of plasticity or viscosity resulting from the union of a nearly perfect fluid with an imperfect solid is seen in very numerous and familiar instances, as for instance in sand, which is itself devoid of any tenacity until its interstices have been saturated with just so much water as to cause it to flow; or in the still more familiar instance of water-ice prepared for the table, in which the varying proportion of the solid and fluid ingredient gives to it every shade of consistency, from a brittle solid to a liquor including suspended solid grains. The prodigious effect of capillary infiltration in determining the motion of even the most solid and ponderous bodies, breaking up their parts, and giving to the motion of the whole a more or less river-like character, is seen in the frequent case of land-slips, as for instance that of Goldau. And scarcely less instructive are the numerous examples, cited in the first section of this paper, of huge masses of almost cold and brittle lavas being pressed on with a uniform and graduated motion, by the almost unimpeded hydrostatical communication of pressure from the yet active fluid which circulates unseen in their pores. With this analogy before me, I replied in 1844 in the following terms to the question, "How far a glacier is to be regarded as a plastic mass?" "Were a glacier composed of a solid crystalline cake of ice, fitted or moulded to the mountain bed which it occupies like a lake tranquilly frozen, it would seem impossible to admit such a flexibility or yielding of parts as should permit any comparison to a fluid or semifluid body transmitting pressure horizontally, and whose parts might change their mutual position so that one part should be pushed out whilst another remained behind. But we know in point of fact, that a glacier is a body very differently constituted. It is clearly proved by the experiments of Agassiz and others, that the glacier is not a mass of ice, but of ice and water; the latter percolating freely through the crevices

* Third Letter on Glaciers, August 1842. Appendix to Travels, p. 407.

† Travels, 1st edit., 1843, p. 175.

‡ Travels, p. 367., edit. 1843. § Ibid. p. 372. ‖ Ibid. p. 361, 372.

of the former to all depths of the glacier; and as it is matter of ocular demonstration, that these crevices, though very minute, communicate freely with one another to great distances, the water with which they are filled communicates force also to great distances, and exercises a tremendous hydrostatic pressure to move onwards in the direction in which gravity urges it, the vast porous crackling mass of seemingly rigid ice in which it is, as it were, bound up*."

Now the water in the crevices does not constitute the glacier, but only the principal vehicle of the force which acts on it, and the slow irresistible energy with which the icy mass moves onwards from hour to hour with a continuous march, bespeaks of itself the presence of a fluid pressure. But if the ice were not in some degree ductile or plastic, this pressure could never produce any, the least, forward motion of the mass. The pressure in the capillaries of the glacier can only tend to separate one particle from another, and thus produce tensions and compressions, *within the body of the glacier itself*, which yields, owing to its slightly ductile nature, in the direction of least resistance, retaining its continuity, or recovering it by re-attachment after its parts have suffered a bruise, according to the violence of the action to which it has been exposed.

The action of warm weather in accelerating the movement of the glacier is plainly due to the abundance of the water saturating its pores; but this may act in two ways; first, by rendering the frame-work of ice less brittle when it is in the very act of dissolving by the circulation of water in a perfectly fluid state through its pores†, and secondly, and more particularly, from the hydrostatic effect of *gorging* a porous mass with fluid. When an incipient frost dries even momentarily the surface of the glacier, the vast porous mass begins to *drain*. This is a very slow process, owing to the resistance to the passage of a fluid through very long and complicated canals. Were it not so, glaciers would be entirely dry after sunset and in winter, which is not the case. The hydrostatic pressure within the whole glacier is however sensibly diminished by the process of drainage; this is evident from watching the level of water in a vertical hole of any depth made within the solid ice of the glacier. After much rain or heat this level is always higher than after dry cold. In the former case the glacier may be said to be gorged, the supply of water from the surface exceeding the power of the drainage to carry it off. The circulating vessels are therefore overcharged. In the latter case the superficial supply is stopped, the drainage goes on slowly though uninterruptedly, and the level of the water in the vertical shaft slowly descends, indicating the diminution of internal pressure. If it were not for the capillarity of the ducts, it is plain that no effective hydrostatic pressure would

* Sixth Letter. Appendix to Travels, 2nd edit., p. 428.

† This I think is undeniable, from the appearance of the collapsed crevasses above referred to, notwithstanding the difficulty of imagining any variation in the sensible heat of water circulating in ice. It is not the only fact in the glacier theory which seems to require some modification of the commonly received laws of latent heat at the very limit of congelation and liquefaction.

be developed at all; the flow being equal to the supply, no part of the *vis viva* would be expended in producing internal pressures. With this concluding observation I commit the Plastic or Viscous Theory of Glaciers to the impartial judgment of those qualified to decide on its merits in explaining facts, and on the variety of difficult and complicated considerations which opposed and still oppose themselves to a complete development of it.

Edinburgh, Jan. 10, 1845.

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Fig. 1. *p. 143.*

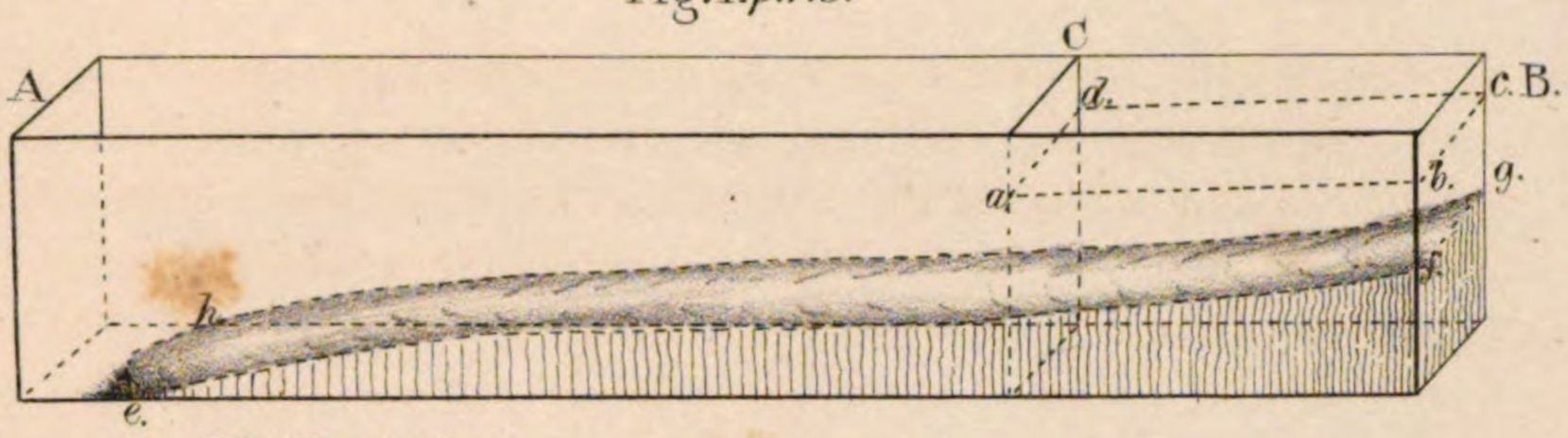


Fig. 2. *p. 144.*



Fig. 3. *p. 141.*

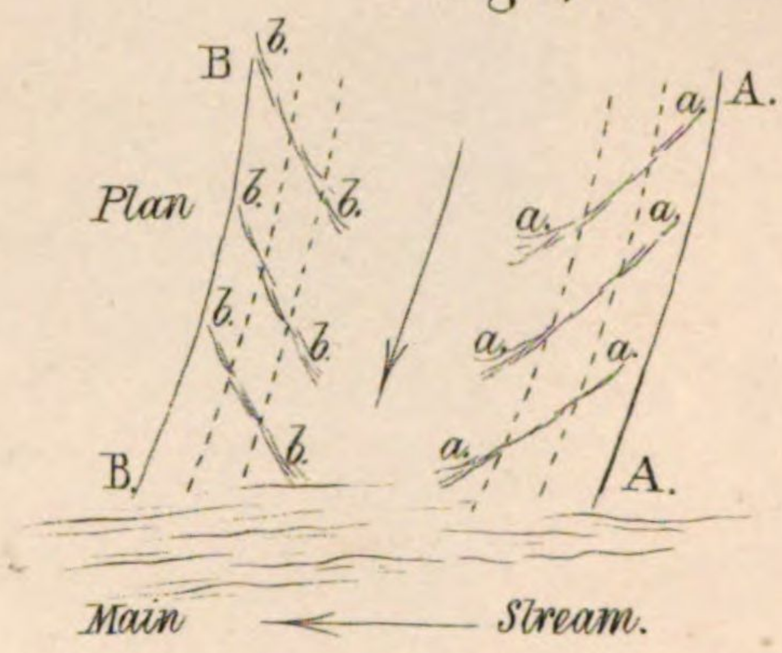


Fig. 5. *p. 151.*

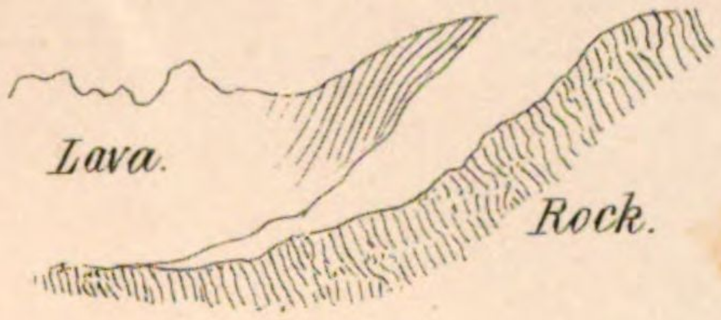


Fig. 4. *p. 144.*



AA. BB. *Sides of Stream.*

a a. b b. Predominant ripple. The dotted Lines shew the direction of floating bodies exactly parallel to the sides.

Fig. 6. *p. 151.*

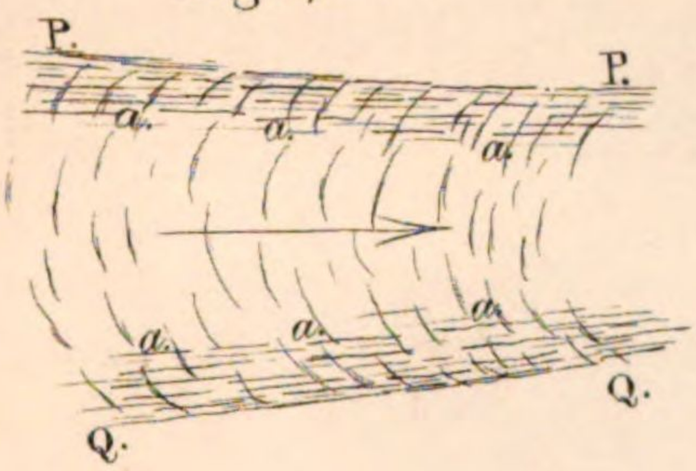


Fig. 7. *Section. p. 151.*

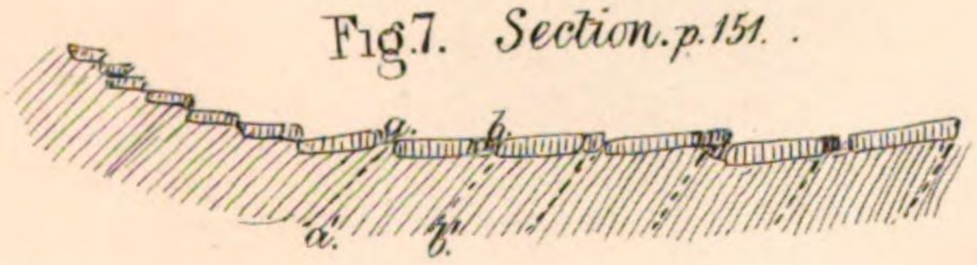


Fig. 8. *Ground Plan. p. 151.*

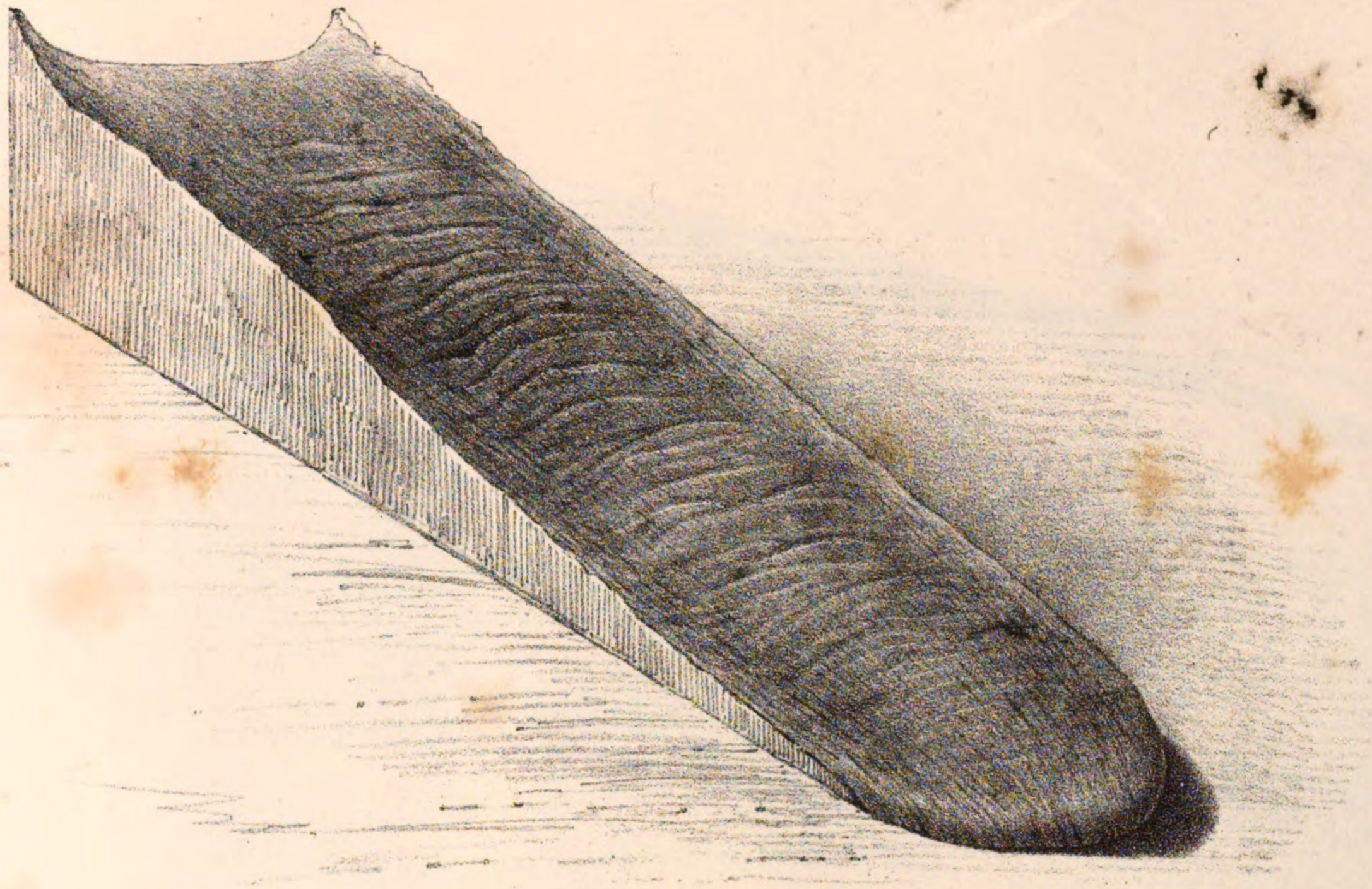


Fig. 9.

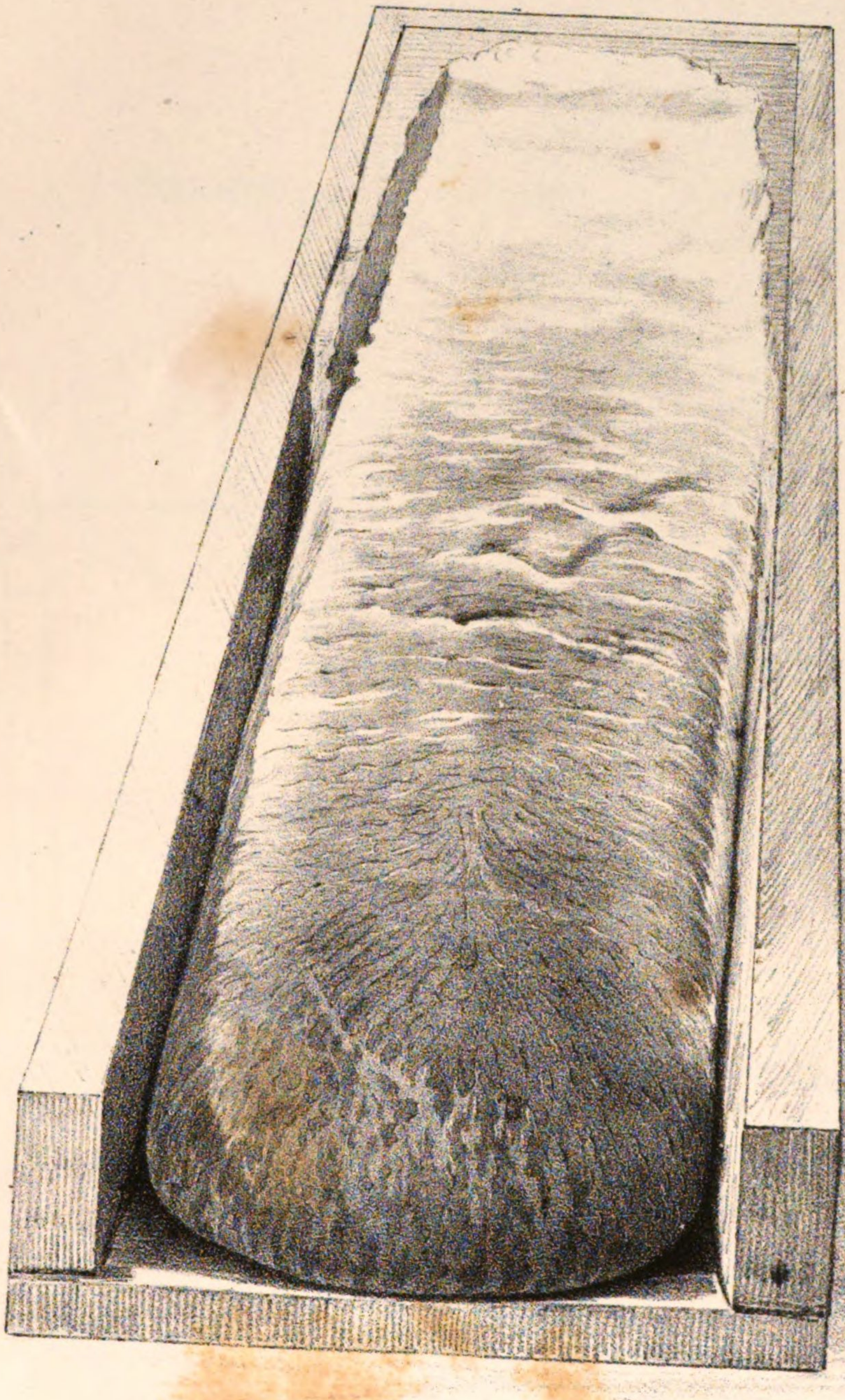


Glacier of La Gria or Bourget.

J. Basire lith.



J. Basire, Lith.



J. Basire Lith.

Fig 1. *p.* 162.

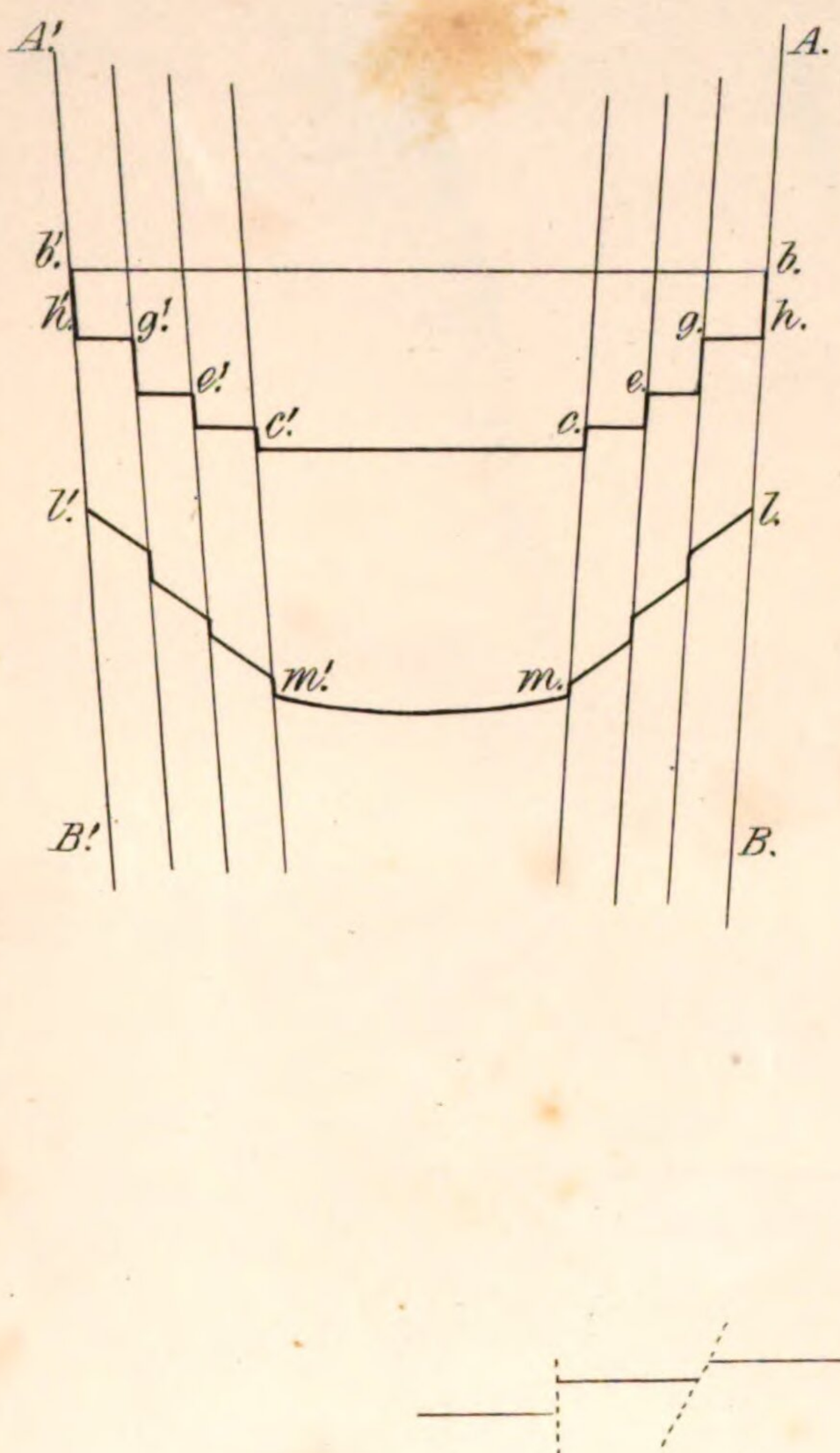


Fig. 2 *p.* 162

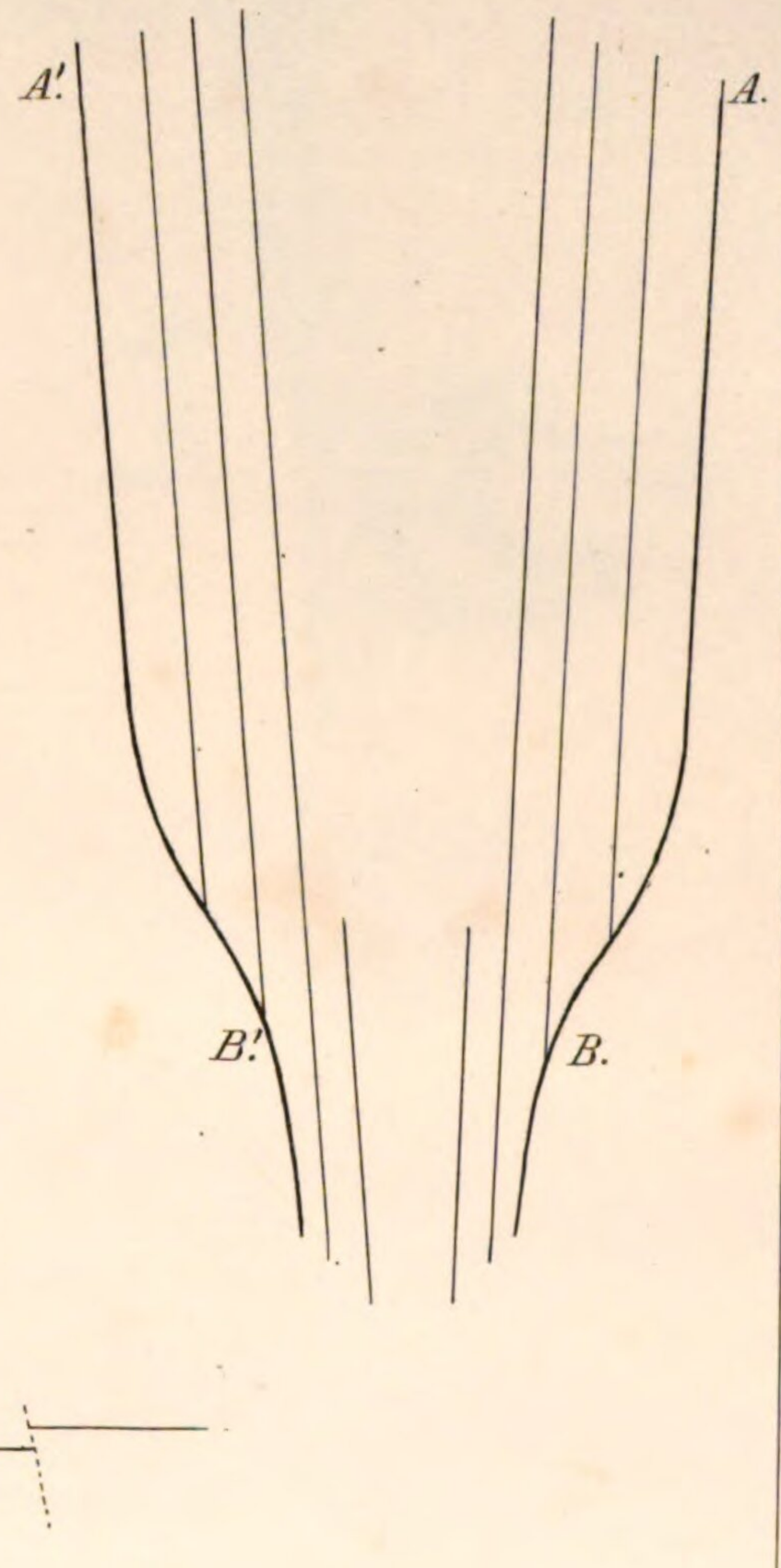


Fig. 3. *p.* 172.

J. Basire lith.

Fig.1. *p.164.*

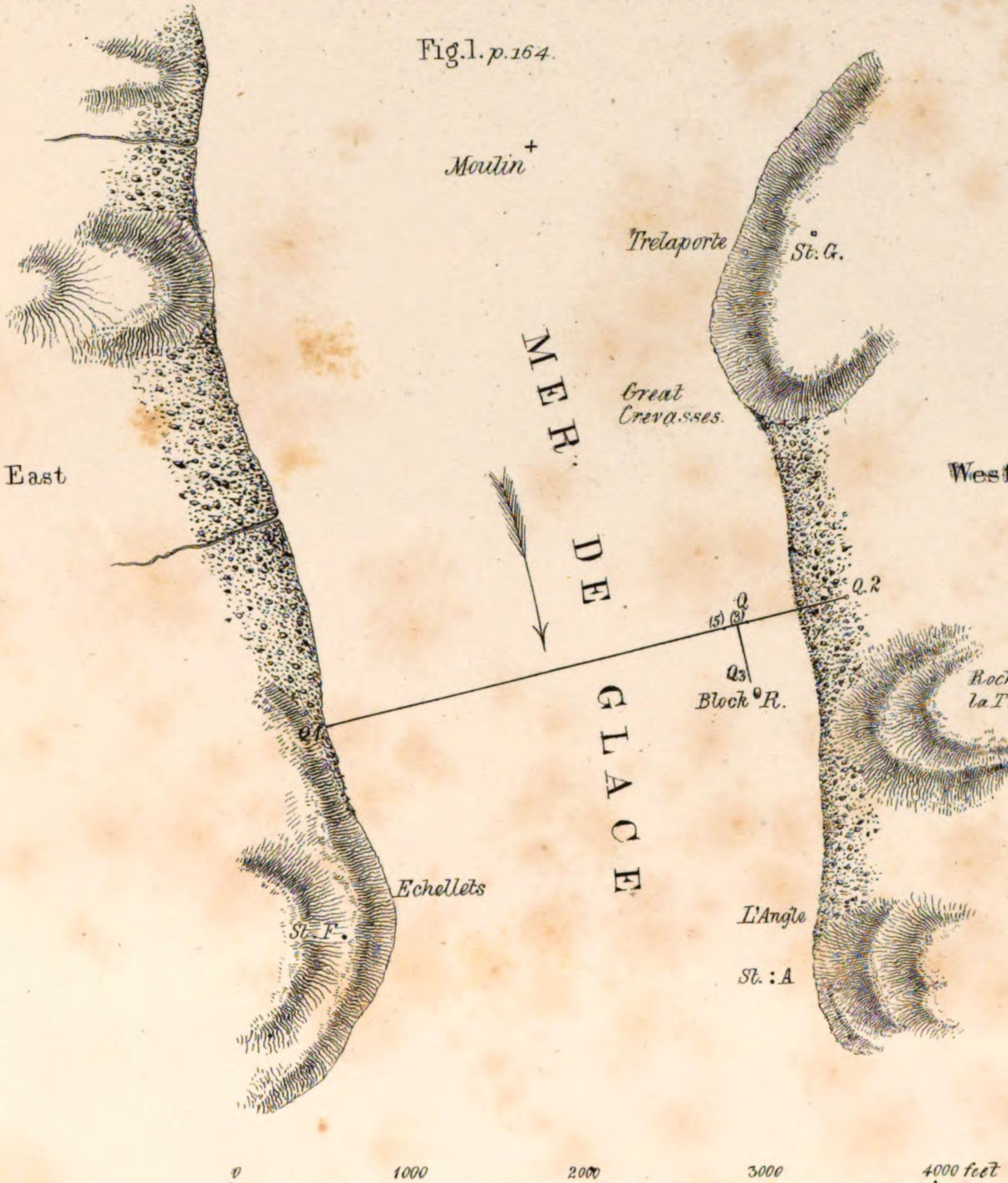
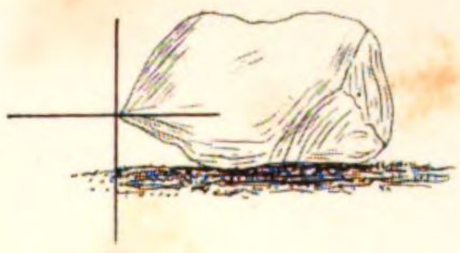
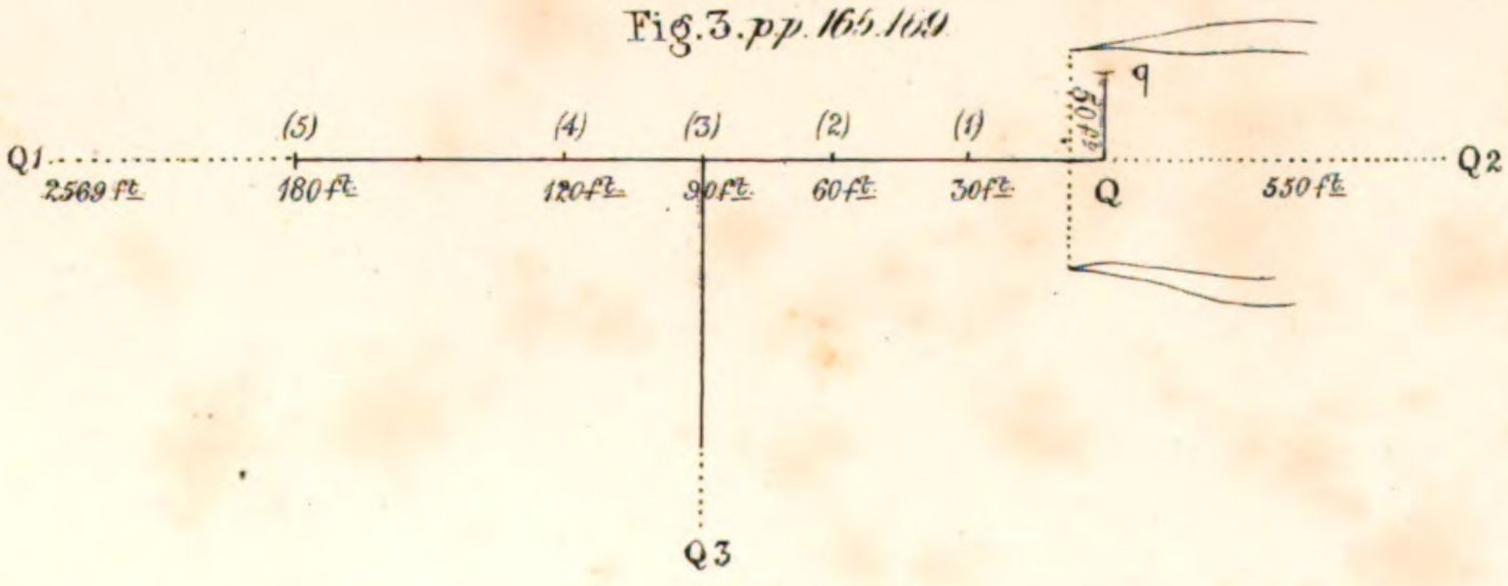


Fig.2. *p. 164.*



Mark on les Echellets.

Fig.3. *pp. 165. 169.*

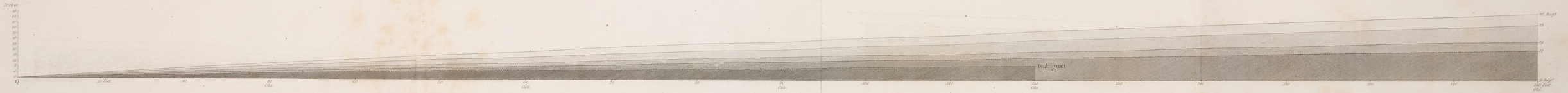


J. Basire lith.

ON GLACIERS PART II. *page 168.*

Fig. 1. Diagram of the Flexure (in a horizontal Plane) of a Transverse Space of the Mer de Glace, 180 feet in length at Station Q, from the 9th to the 26th August. From observations made at 6 points viz.t at 0, 30, 60, 90, 120 & 180 feet.

Horizontal Scale ..... $\frac{1}{40}$ of Nature
Vertical Scale (Corresponds to forward Motion) $\frac{1}{40}$



ON GLACIERS PART II. *page 172.*

Fig. 2. Diagram of the Flexure of a Transverse Space of the Mer de Glace 90 feet in length at Station Q, from the 20th to the 25th August 1844, observed at intervals of 2 feet.

Horizontal Scale $\frac{1}{40}$ of Nature. — Vertical Scale (Corresponds to forward Motion) $\frac{1}{40}$



J. Basire sc.

Fig. 1. *p.178.*



View of the Glacier of the Schonhorn-Simplon.

Fig. 3. *p.201.*



N°1. SPRING.

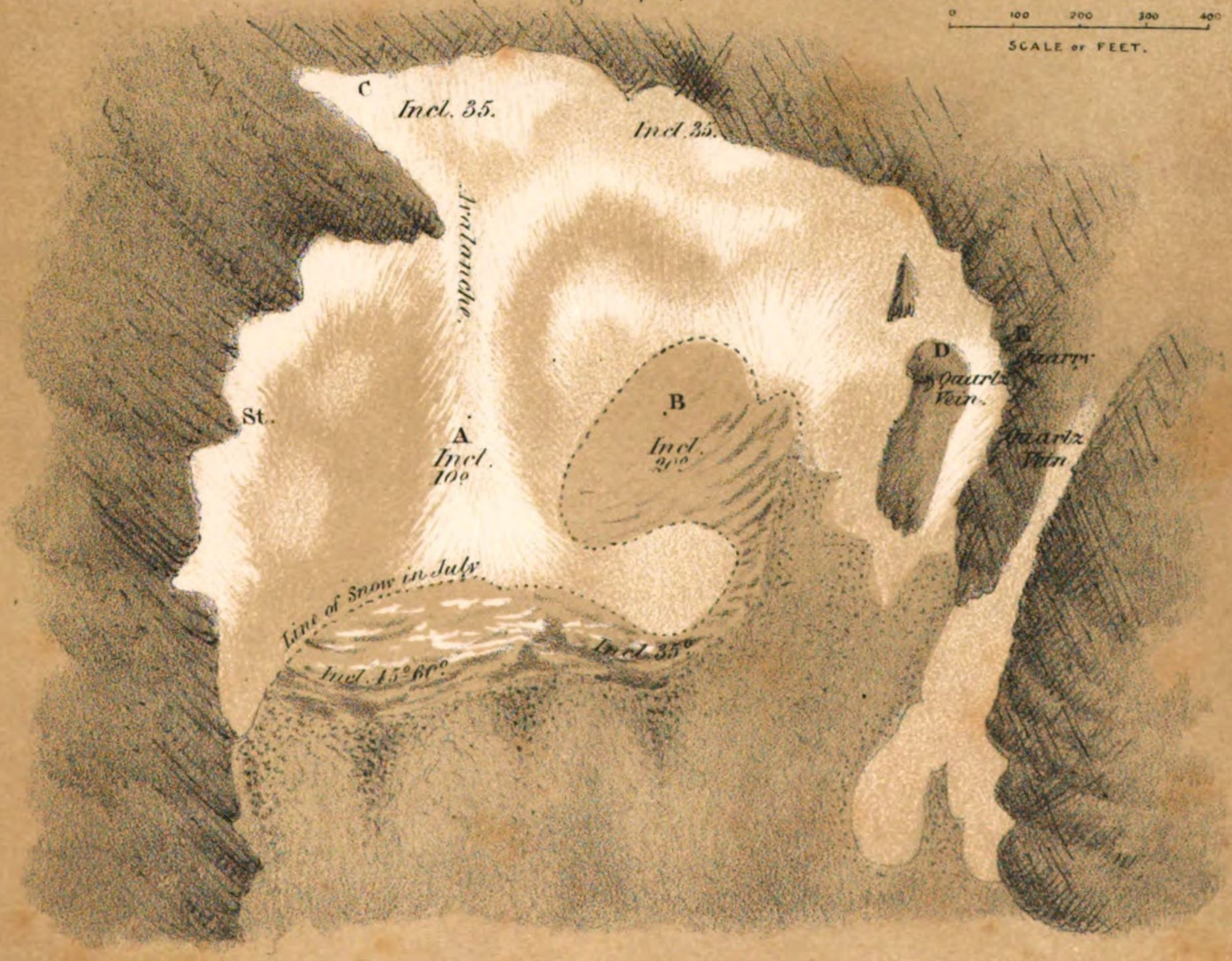


N°2. SUMMER.



N°3. AUTUMN

Fig. 2. *p.178.*



Eye Sketch for a Ground Plan of the Glacier of the Schönhorn-Simplon.

J. Basire, del.

FIG. 1. *p. 186.*

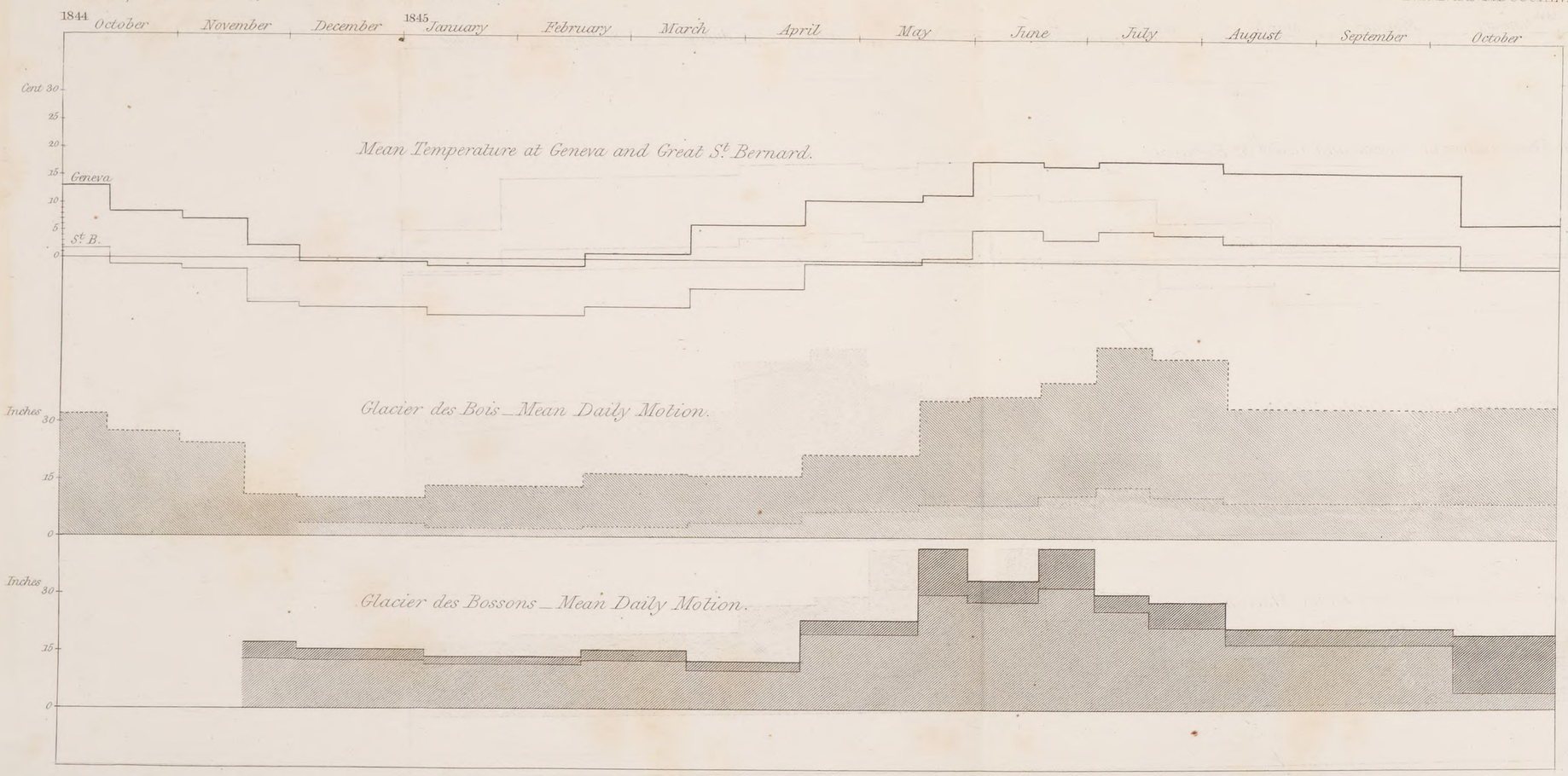


FIG. 2. *p. 191.*



J. Basire sc.

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Forbes, Jam. D.

Illustrations of the viscous theory of glacier motion

London 1846

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